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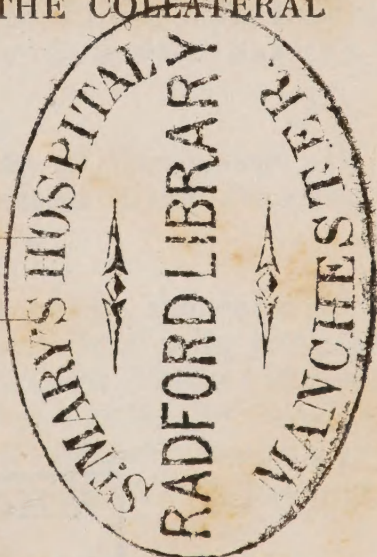
THE
DUBLIN JOURNAL

OF
MEDICAL SCIENCE;

EXHIBITING
A COMPREHENSIVE VIEW
OF THE
LATEST DISCOVERIES

IN
MEDICINE, SURGERY, AND THE COLLATERAL
SCIENCES.

VOL. XVI.



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The following Works have been received :—The Medical Portrait Gallery, Part XX.—The First Number of the Medical Miscellany, (London, October, 1839.)—The Medical Examiner, (Philadelphia).—La Gazette Medicale, (Paris).—The New York Journal of Medicine and Surgery.—Illustrations of Midwifery, by M. Ryan, M.D.—Tabular View of the Signs furnished by Auscultation and Percussion in Diseases of the Heart, by O'Bryen Bellingham, M.D., (Dublin).—Outlines of Comparative Anatomy, by R. E. Grant, M.D., &c. &c., Part V.—First Report of the Council of the Pathological Society of Dublin.

Dr. Elliotson's Work on the Practice of Medicine will be reviewed in our next Number.

ERRATA.

In Dr. Evanson's Paper on Vaccination, in our September Number, the following corrections are to be made :—

Page 22, last line, *for city, read quarter.*

— 30, line 6, *for last, read lost.*

— 30, — 6, *for required read regained.*

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The length of the original communications in this Number has prevented our inserting Dr. Doherty's Paper, entitled "A further Reply to Mr. H. Carmichael's Views on the Position of the Placenta, &c., in which is given a Statistical Table of its Situation in 100 Cases, as determined by the Stethoscope, and the Measurements of the Membranes." It will appear in our next Number.

We beg to acknowledge the receipt of several recently published works, notices of which shall be inserted on the first opportunity.

Dr. Graves feels much pleasure in being able to announce, that through the exertions of Sir James Clark, a copy of a most valuable document, relative to the cholera, has been discovered. It consists of a Report made by the Commissioners to the King, and it details the dates of arrival and cessation, the number attacked, and the proportion of deaths to the number of inhabitants in each place, contained in the list published in Dr. Graves' present Paper. This document will be published in the next Number of this Journal, and will furnish a most minute detail of Cholera Statistics in Great Britain.

THE
DUBLIN JOURNAL

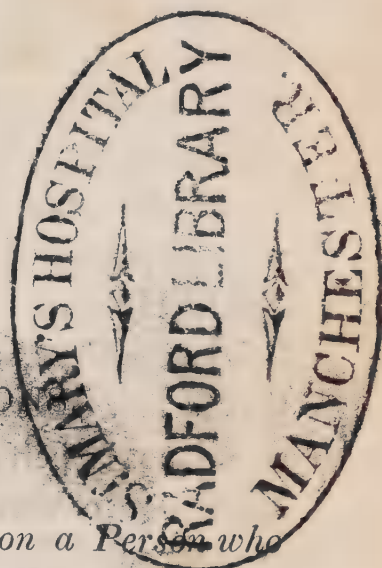
OF

MEDICAL SCIENCE,

1 SEPTEMBER, 1839.

PART I.

ORIGINAL COMMUNICATION



ART. I.—*Anatomico-physiological Remarks on a Person who had been beheaded.* By Dr. THEOD. LUD. WILH. BISCHOFF, Professor at Heidelberg.

[Condensed from Müller's Archiv. by S. L. L. BIGGER, M. B., L. R. C. S. I.]

ON the 6th of July, 1838, Sabastian Zink, a robber and murderer, was publicly beheaded with the sword. My friends and colleagues, Drs. Heerman and Jolly, determined with me to take advantage of this opportunity in order to elucidate many scientific questions, if possible, and draw some advantage from this spectacle, so melancholy to every philanthropic mind, although beneficial to mankind in general, according to the best of our abilities. Dr. Jolly had the kindness to furnish us with many of his far-famed physical instruments, and I did not forget my Schick's microscope; thus were we armed to put in execution and fulfil the plan which we had laid down.

We thought that it would be well to institute some observations on the existence and probable duration of consciousness

in the head when separated from the body ; although it is probable that the greater part of our readers are familiar with the very accurate and critical paper by Professor Nasse the elder, in his “ *Untersuchungen zur Physiologie und Pathologie*,” published at Bern in 1835, on this subject. All the questions entertained in that paper are answered in the most accurate manner ; and as it contains besides a compilation of all the observations which had been made hitherto on the subject, it served me as a starting point, where I could take up the subject without entering into the details of all that has been done.

Phoebus, almost at the same time with Nasse, published a very important collection of facts in Hitzig's *Annalen*, Part XXXIII., and also in the *Berlin Encyclopædia of Medical Science*. He gave as his opinion, (a little hastily we think), that further observations on this subject were unnecessary, and should not be permitted. However, it appeared to me that our knowledge of nervous action and its connexion with the action of the mental faculty had not yet arrived at so perfect a pitch, and in latter years the change had been so great in our ideas of the nervous system, that a new field of inquiry lay open for the institution of new observations which appeared to me well worthy of the trouble.

[I omit a good deal here of introductory matter, and proceed to the description of what occurred in these experiments.]

It was not without difficulty that Dr. Heerman and I obtained leave to occupy the place immediately under the scaffold, so that we should have the head and body of the decapitated man immediately under inspection, whilst Dr. Jolly remarked on the outside every thing which should occur, the greater part of which we could also both hear and see.

A few seconds after the blow with the sword had been struck, at thirty-six minutes and a half past nine, we received the head, which was hastily handed down in the bandage which had covered the face, without having been deranged by falling

or receiving any shock. The bandage was immediately removed in order to see the manner of the separation from the body, the bleeding, and the impression of the features. The blow had been given most successfully, and the blade had passed anteriorly between the os hyoides and the larynx, posteriorly very nearly between the fourth and fifth vertibræ, yet in such a manner that a portion of the left oblique process, and of the body of the fourth and a piece of the oblique process of the fifth were hewn through. The blood flowed slowly and continually. The expression of the countenance was of the most perfect tranquillity, not a trace of pain or distortion. The eyelids were a little sunk, the mouth closed yet easily opened. The expression of the eyes was perfectly tranquil, neither staring, as mentioned by an earlier observer, nor yet dull and without lustre, but like those of a person who looks at something at a great distance. Dr. Heerman who had visited and sat with the criminal for an hour on the preceding evening, could not discover any greater change in the features than the absence of his accustomed piercing sharp glance and the pupils being somewhat dilated.

I approached my fingers, and then a brilliant needle close to the eyes yet without touching either eyes or eyelids. This was repeated often without causing the slightest motion in either the eyes, eyelids, or other features.

Dr. Heerman now shouted his name in his left ear, and the word "pardon," under the conviction that this sound would produce an effect upon whatever consciousness might be present, as up to the last moment the criminal had been continually hoping for a reprieve. There was no trace of any motion. I now held to his nose a bottle of very strong smelling tincture of assafoetida, which preparation I preferred to ammonia, because I expected a purer action from it on the olfactory nerves, as it might be possible that the ammonia by stimulating the nerves of the mucous membrane should produce a motion of reflection. This experiment also failed. I then dropped upon

his tongue a drop of the tincture of colocintida; a feeble thrusting forward of the tongue and slight motion of the jaw ensued, both these motions were repeated three or four times at short intervals, whilst in the other features no trace of motion was visible. In order to try in a certain degree how far the spirits of wine contained in the tincture could be regarded as cause of the motions, I placed a drop of pure spirits of wine on the tongue, which caused a similar motion of both tongue and jaw.

All these experiments were finished within one minute after the fatal blow had been given. The question now arises, were those feeble motions of the tongue and jaw the consequences of sensation, if so, consciousness must have been present; or were they reflected motions dependent on irritation of the mucous membrane of the tongue, or from irritation of the severed spinal marrow, and therefore not caused by the tincture of colocintida? To both of us the last of these suppositions appeared most probable. The tranquil condition of all the other muscles of the face appeared to contradict the idea that any unpleasant taste or flavour had caused these motions. The symptoms of the reflex function, however much developed in the motions of chewing and swallowing, are much less affected by irritants applied to the tongue than by those applied to the mucous membrane of the palate, uvula, and jaws. I must mention also that real irritation of the mucous membrane of the tongue with the point of a pin produced no motion. On the other hand, opening and shutting of the mouth are the motions which are most frequently to be seen in decapitated animals, and which are certainly dependent on the violent irritation of the spinal marrow. The tongue can move very little at the same time, on account of the attachments of the muscles which retract it having lost their fulcrum.

Thus there does not appear to have been a single symptom present which permits us to suppose that consciousness was present; it appeared to have ceased instantaneously. It is right to

remark that previous to the blow being given, he was not at all confused in mind, as were the ten criminals beheaded by the executioner Brand, as communicated by Heim. Zink, previous to execution, was in a perfectly careless mood, yet a little excited, and displayed the lamentable rudeness of his general demeanour; his mental powers were so little affected, that he said to the attendants of the executioner, as they were binding on the bandage on his eyes, an instant before he received the blow, "ye slaughter one like butchers!" During life this man seemed to possess very little nervous sensibility, the same was evidenced after death in the experiments performed, so the evidence of those who have obtained contrary results is not by this instance much weakened. Yet I may say that negative results of an observation in the state of mind in which the greater number of observers must be at such a moment, carry with them more credence than positive ones, in which the excited fancy may play a prominent part, as from the relations of others, who during similar operations previously have beheld marvellous things. All the evidences brought forward with any accuracy to prove the continuance of consciousness have consisted mainly in a few remarks, on impressions being conveyed to the auditory apparatus in animals which had been irritated, which collectively cannot be considered as motions of reflexion, or as caused by irritation of the spinal marrow.

I now proceeded to perform certain experiments from which I expected real motions of reflexion. I touched with a needle the eyelids and eyelashes as well as the conjunctiva; I irritated the mucous membranes of the nose, mouth, and pharynx, in the expectation of perceiving motion, but all in vain; all the muscles remained perfectly quiet, so that this was a proof not only of the rapid disappearance of consciousness, but also of that of the nervous irritability. Even piercing the severed part of the spinal cord with needles, and touching it with a pencil of kali causticum, produced no motions more in the head, whilst yet not more than from two to three minutes had passed from the period when it was severed from the body.

We now turned our attention to the trunk, and whilst Dr. Heermann quickly tied the carotids, out of which the blood continued to flow with little jerks, in order to support the nervous and muscular irritability by detaining the blood, I sought, by piercing, scratching, and pinching the skin on the soles of his feet, fingers and toes, to call forth some reflex motions; this proved vain also. Irritating the spinal cord produced twitchings in the pectoral muscles, and elevation of the arms.

The corpse was now placed in a coffin and conveyed to the neighbouring hospital, where every thing was ready for further experiments. It was now fifteen minutes past 10, consequently thirty-eight minutes and a half since the execution.

We next introduced some electrical streams into the nerves. For this purpose Dr. Jolly had constructed a galvanometer, which, by the immersion of a single plate of one-fourth inch square in pure water, produced an aberration of the magnetic needle to about 90° . I first sunk the platina terminations of the two conductors, one in the grey, the other in the white substance of the spinal marrow of the trunk. This process being repeated often, and the needles changed, not the slightest motion of the magnetic needle occurred; the introduction of the needles also caused no twitchings of the muscles. This experiment was tried on account of the assertion of Folchi, who stated that in a calf which had just been decapitated, and on which a similar experiment had been tried, there was an aberration of the needle about 6° west, on every new introduction of the needles, (Froriep's Notizen, No. 950,) which experiment had often before been tried without any effect on decapitated dogs. Dr. Heerman now exposed the median nerve in the upper arm, and I plunged the two needles into the trunk of the nerve at the distance of about one inch from one another, and endeavoured by mechanical irritation of the spinal marrow to produce twitchings in the arm; none occurred, nor were there any changes in the magnetic needle. I now applied one pole of a powerful galvanic battery of sixty pair of four inch plates, capable of giving me

a tolerable strong shock, producing sparks, and decomposing water, upon the spinal marrow, the other on the hand. There arose evident yet slight twitchings in some of the muscles of the fore and upper arm, viz. in the supinator longus, extensor carpi ulnaris longus, and in the internal head of the triceps, but no motion in the magnetic needle. It also remained perfectly unmoved when I applied the second pole upon the trunk of the nerve, the same was the case when I applied both poles to the trunk of the nerve, so that the chain was perfect, although in the latter instance twitchings occurred in the muscles mentioned above. Some inference may be drawn for the hypothetical electrical streams in the nerves, at least that the nerves are exceedingly good conductors of electricity, and much better than the metals, or otherwise the magnetic needle should have been affected in the last case, had it been the better conductor. Mattevic in the *Bibl. Univers de Genève*, August, 1834, has communicated a similar experiment with like result. On the contrary J. Müller has, in the *Archiv. für Anatomie, Physiologie, &c.* declared that all parts of the body, and even a drop of water act in the same manner. Without wishing to raise any doubt against this observation, we may say that from it can not be deduced that the nerves and perhaps other moist organized parts are not good conductors. The want of reaction in the galvanometer, when the circle is completed, by any of these parts will admit of the general conclusions being drawn, either that they are very good or very bad conductors of electricity. The experiment of Müller admits only the one explanation, that the moist organic structures are very bad conductors, for he only employed a weak source of electricity, a single pair of moistened plates and the circle was not completed, and thence there was no aberration of the magnetic needle. On the contrary in our case the occurrence of reaction, the contraction of the muscles shewed that the circle was completed. Should any one object that nerves are bad conductors, and that the results we obtained were from our using a very powerful source of electricity, his own

argument might be used against him, for the more powerful the electricity the more likely would it be to pursue the metallic conductor if it were the best. The non-reaction of the galvanometer appears to have been caused by the very great conducting susceptibilities of the nerves (or perhaps of the other animal tissues,) for electricity. I do not wish to draw as a conclusion that electricity is the active agent in the nervous system, a circumstance which for many other reasons I think to be improbable. I have repeated also the experiments of Varvasseaux and Berardi, as well as those of David, and in no instance has there been any change of position remarked in the magnetic needle when I sink the platina conductor of the galvanometer into the nerves of a living animal, and by means of mechanical irritation caused motions and twitches, actions of nervous irritability. This is accounted for, as we said before, from the nerves being better conductors than the metals.

Mechanical irritation of the median nerve, pricking, pinching, and even cutting it across, produced no effects, whilst galvanism still produced twitchings.

I next opened the chest and abdomen, whilst Dr. Heerman tried some experiments on the irritability of the iris. It was now fifty minutes past ten, and the iris showed no disposition to contract, not even when the pole of the battery was applied directly to it, the cornea having been removed, yet the battery was so strong that it immediately caused decomposition of the fluids of the eye, and a development of gas.

In the chest and abdomen there were no spontaneous movements to be seen; also when the galvanic pole was applied consecutively to the phrenic and vagus nerves, to the stomach and intestinal canal, on the ureters, gall bladder, and the cystic duct, no contractions occurred. The right auricle of the heart alone continued to move after this, for an hour and quarter, the same occurred in the muscles of voluntary motion, but was scarcely perceptible after an hour and fifty minutes. As the experiments on the phrenic and vagus nerves were made at ten

minutes past eleven, just one hour and thirty-three minutes and a half from the execution, even then the twitchings in the voluntary muscles were very weak, in comparison with those mentioned by other observers in other cases, which confirms the remark which I made already with regard to the head, that the nervous faculty and capability of irritation were not strongly developed in this man.

Whilst these observations were making, I was seeking the thoracic duct, which I found, with a good deal of trouble, an account of the body being very fat and the vessel containing only a muddy greyish fluid: a portion of this fluid being removed, coagulated as usual, but it formed a very small coagulum around the bit of stick which was moved in it. Under the microscope the little round bodies did not appear numerous in the fluid, but were nearly of the same size as the molecules in the blood, many of which might have entered along with the chyle whilst we were obtaining it, but there is sufficient to distinguish these two sets of bodies from one another.

I take advantage of this opportunity to communicate my observations on the chyle molecules which I had often examined in dogs. In the white chyle of the thoracic duct of young dogs, after being fed, I have always seen two kinds; the first exhibits an innumerable quantity of exceedingly small little bodies (*körnchen*) or particles which are only to be seen when the chyle is permitted to run on plates of glass, when their appearance is exactly like that of sand put in motion. The microscope must be very powerful to shew this. The second kind of little bodies are fewer in number, much larger, and the greater part of them of the size of the molecules in the blood, but there is no kernel and no envelope to be seen in or on them, besides they have not the yellowish appearance which, when powerfully magnified, even a single blood molecule exhibits; they are unchangeable in water and in acetic acid. When the chyle is agitated they sink to the bottom, and they form a part of the coagulum. The rest of the fluid remains white, where they are

in very small number, and where they could not be the cause of the intense white colour. This colour is, I think, attributable to the other set of innumerable little molecules which I take to be fat. Still I am opposed to the opinion of J. Müller, that the white colour of the chyle is principally attributable to fat; for if the chyle be treated with æther in a glass tube, often shaken, and fresh æther still added, (not in a watch glass where the experiment cannot succeed,) it loses nearly all its colour, and appears of a light opal. This remnant of opacity cannot be caused by the larger molecules, for they were dissolved just as the blood molecules are by the æther, a process which I saw going on; but it seemed to me that this opacity arose from coagulated albumen, which is more difficult of solution in æther. Very numerous little bodies were to be seen in the fluid which remained like coagulated albumen. I believe that the evidence of Tiedeman, Gmelin, and J. Müller are alike, all considering the prevalence of the white colour in the chyle as proceeding from very minutely divided particles of fat, but that these are not the genuine chyle molecules which exist independent of them. This view is strengthened by this, that in the pinkish contents of the duct in dogs which had fasted long, those very fine particles were not so evident, whilst the other chyle molecules were even as numerous, nay relatively more numerous than usual.

After having collected the chyle, I observed the mucous membranes of the trachea and larynx, and had the pleasure of seeing myself, and being able to shew to those present, the ciliary motions. These motions were not discoverable in the œsophagus, not even in that part which covers the posterior wall of the cricoid cartilage.

I next examined the urethra, vesiculæ seminales, vas deferens, epididymus. In the urethra it was evident, that an ejaculation of semen had taken place, as in the case given in Valentine's Repertorium, I. p. 277; this arises, naturally, from the violent contraction of all the muscles at the moment of de-

capitation. There was found in the urethra, many large yellowish coagula like coagulated lymph, and a whitish fluid which contained mucous seminal animalculæ alive, but not so numerous as those which I have seen in the seminal fluid of other animals. The same kind of coagula, and the same animalculæ were seen in the vesiculæ seminales, which were of small size. The animalculæ were found in the whole course of the vas deferens alive, but not in such great numbers. I thought I could see them also in the seminal canals of the testis, but as they were few in number, and not living, I do not wish to assert this as a positive fact. The contents of these parts contained also many other kinds of particles, but none of such determined forms as those which Valentine describes. It is probable they were particles of epithelium. The fluid which exuded, on a section of the prostate being made, was clear and pellucid, but I could not discover in it any elementary bodies, except some blood molecules.

Much pains were taken to examine the brain carefully; when the scull-cap was removed, all the veins were found to be full of air, and air had penetrated between the pia mater and arachnoid, causing a very peculiar appearance on the surface of the hemispheres. Could this air have entered at the severed part of the spinal cord? This is difficult to credit, for in the brain as well as in the spinal cord, the pia mater and the arachnoid lay very close together, and no fluid intervened, the escape of which, might permit the entrance of air. Then whence came this air? Through rupture of the vessels? This I am not willing to allow either. And by this my view is strengthened, that the formation and course of the arachnoid is not yet perfectly discovered. The examination of the brain was not satisfactory. Particular attention was paid to the spleen: in it I found the little white bodies described by Malpighi, which I have seen in the four classes of vertebrated animals, but which are hard to discover clearly in man. I believe that the function of the spleen is to produce them, and I cannot see any

reason why Müller should suppose, that those he has seen in certain ruminating animals, are of a nature different from those which have been found in the spleens of other animals, or from those which I believe to exist in the healthy spleens of all men. That these are hard, and tolerably large in some, whilst in others they are soft, small, and easily dissipated, is no reason that they should be considered essentially different. I found also in these bodies, universally, as well as in the spleen of this subject, those little round bodies or balls which J. Müller has described. With a cataract needle it is easy to isolate as many of these little bodies from the spleen as may be necessary for microscopical observation. I find that these are exactly similar in appearance, size, relations to water and acetic acid, to those of the chyle; therefore is it not possible, that these may originate in the spleen? It is true that the corpuscles of which the parenchyma of the liver consists, are, as Müller remarked, very like those in the corpusculi Malpighiani, yet similar bodies are to be found in many places, and still the parts organized from them, are very different from one another.

From attention to this point, I think shortly, much information will be obtained with regard to the spleen, and the formation of the corpuscles of the blood, although as yet no change has been seen in the formation of blood after extirpation of the spleen. It is questionable whether sufficiently accurate examination has been made of the blood globules in those animals which have been deprived of their spleens.

Lastly I examined the stomach and the villi of the intestinal canal. In the former the mucous membrane of the fundus was strikingly softened: it was now 4 o'clock, and the day very warm, the contents of the stomach smelt very sour, from wine and salad. The villi of the small intestine were of pyramidal form, formed of little bodies united together, and I could not discover any epithelium formation on them after they had been gently washed in water.

There was no diseased appearance in the whole body, except an adhesion of the lung to the walls of the thorax, occasioned by an old gun-shot wound in all probability; three grains of large shot, were found under the pleura costalis, unchanged in appearance and surrounded by a quantity of cellular tissue.

The ventricle of the heart was very strong and muscular, and compared with the rest of the muscular structures, might be called hypertrophied.

After six hours busily and uninterruptedly employed with the body of this unfortunate man, though aided by my friends, I still feel that many observations might have been made in a more accurate manner. My hope, is that what has been done may lead to further observation on the same subject.—*Archiv. für Anatomie, Physiologie, &c. Von Dr. Johannes Müller, No. V. 1838.*

ART. II.—*A Letter on the Question of the contagious Nature of Asiatic Cholera, addressed to the Editors of the Dublin Journal of Medical Science.* By EDMOND SHARKEY, M.B., T.C.D.

Is Cholera Infectious?

TO THE EDITORS OF THE DUBLIN JOURNAL OF MEDICAL AND
CHEMICAL SCIENCE.

BERE HAVEN, *May 13th*, 1839.

GENTLEMEN,

Some high authorities have objected to the discussion of this *verata questio* of medical polemics, on the score that even were the affirmative tenable, its “withering influence in case of a second outbreak of the disease,” ought to deter us. This is an opinion the soundness of which is I think very questionable; and I am induced now briefly to consider the subject, in consequence of the views given upon it by Dr. Budel and

Mr. Busk, in their Report of Cholera on board the Dreadnought; which Report was reviewed in the Medico-Chirurgical Review for October, 1838, believing that the epidemic of last year (still so fresh in my memory) furnishes some facts tending strongly to shew at least that the non-infectious nature of cholera is far from being established. In the discussion of questions respecting infection, uncertainty as to the precise import of the terms, no less than diversity in the classification of infectious agents, and the disease produced by them, has ever given rise to confusion and difficulty, and distinctions frequently grounded on differences too unimportant, have very much embarrassed and continue to embarrass the subject. When it is considered that in a state of health, the animal body is constantly engaged in various processes of elimination, failing which, the most formidable derangements of the system ensue, as familiarly exemplified in jaundice and urinary suppression, it will readily appear that principles unfriendly to life, when eliminated in the shape of impalpable effluvia may, even in the case of a number of healthy persons crowded together, acquire such a high degree of concentration as to produce dangerous diseases, shewn by medical history to be in many instances capable of self propagation. It must be evident from hence what difficulty there is in determining what diseases may properly be included in the large class of *conditionally infectious diseases*.* Indeed it may I think be questioned whether more can be predicated of fever, or any other confessedly infectious disorder, than that it is conditionally infectious. How else can we account for a single escape after exposure? In the few observations however which I shall make, I will merely inquire, "is cholera one of those diseases, which under ordinary circumstance of atmospheric condition, unrestrained intercourse, and common attention to cleanliness and ventilation, have a tendency to produce the same disease in healthy persons in close neighbourhood to the sick?

* Vid. Copland's Med. Dict. Art. "Infection."

In other words, whether or not a person not predisposed to the disease, either by nervous temperament or epidemic influence, may as safely enter the apartment of a cholera patient as of one labouring under bronchitis or rheumatism." The whole marrow of the controversy is, I think, contained in this. In the solution of the question, it is needless to say, the interests of society are deeply involved. If it can be answered in the affirmative, all quarantine restrictions are a wanton outrage on the commercial interests of a people; if in the negative, all attempts to do them away or soften their rigor, are so many efforts to sacrifice thousands of lives to the Moloch of national aggrandizement. I will first state some circumstances which have fallen under my own observation, and secondly examine some of the conclusions drawn in the Report alluded to, conclusions, as it appears to me, not warranted by the premises. First, then—seventy six of the cases which I had charge of were thus disposed. They occurred in twenty-eight houses, sixteen of which had two cases each, eight had three, one had four, two had five, and one had six. One village (Ballydonagan,) whose population was 493, and which consisted of ninety-two houses, had thirteen infected ones, of these four had two, four had three, two had four, and one had five cases. Another (Lahanbeg,) having a population of 310, in fifty-five houses, had ten infected ones, in which the cases were as follow, viz. two in two, three in one, four in one, and five in one, the others had single cases. The village of Clogheen, forty houses, population 207, only two infected houses, in one of these, three cases. How unlike this to sporadic or uninfected epidemic disease!

PARTICULAR CASES.

A woman in the village of Lahanbeg, was attacked by the disease and recovered. One of her children was removed to a neighbouring village, an uninfected one; after having been there for several days it was attacked, no one in the place being ill then *or subsequently*. This child was

removed to the house from which it had originally come, and there died. Two other children in the same house also took it and died; and lastly, an old woman came down from the same uninfected village to the same house to see them, and there took it and also died. There was not at this time another infected house in the village of Lahenbeg.

CASE II.—Three persons, a young man, his wife, and a young girl, æt. 16, having been alarmed at the occurrence of a cholera case in the house in which they lived, took flight, and came a distance of about nine or ten miles to this same village of Lahenbeg, to a house not previously infected. The three were attacked that night. The man and girl died, the latter after a very few hours. Subsequently the infant of the man and woman took it, but recovered; and afterwards the man's sister, who had been suckling the child, died after three or four hours' illness; lastly the old mother who had been in attendance on them all, was slightly attacked. It is right to mention that the first three had slight diarrhoea at the time of their flight.

CASE III. occurred in the village of Clogheen before mentioned. It was the first in this village, and was that of a little girl who had been living for a month before in a village about three miles off. A case occurring in the house with her, she came home, and was attacked in three or four days after. The only house to which the disease extended besides, was the one next to it, in which three cases occurred. Both these houses were at some distance from, and elevated above, the main body of the village. In the latter house the dog and cat were said to have died with symptoms of the disease; and I may mention that the same occurred in the case of a parrot, belonging to a brother physician, at the time when cholera was raging about him.

CASE IV. occurred in the practice of my valued and able friend Dr. Philip A. Armstrong, of Castletown. A lady was attacked by the disease, and recovered; a little girl was next attacked in the house, and after her the grandmother, who died; after her the girl's father, also resident, who died. At this time the lady first attacked caught fever, and died her

“wake,” was kept up for two days, and of those who sat up at the wake in that house, five were taken ill together; amongst them the little girl, a second time. Of the five two died, finally three others, who had been engaged in nursetending the former, also took it and died. I very much doubt if in the whole history of fever, a stronger case than this last could be found. I will now proceed to consider some of the objections urged in the Dreadnought Report, against the infectious hypothesis. It is said that the first case, that of the man from Dantzick, “could not have been the result of importation, because he came over in a ship in which no case occurred, and the disease could not have been dormant so long.” Now first—As to his having so long remained in the other ship, without having communicated it. How could he have communicated a disease not yet developed in himself? We could not expect to communicate vaccinia from a subject in whom it had not yet formed itself? And on this point I may observe that I have known more than one instance in which a person apparently uninfected, fled from an infected to a healthy place, and, after the lapse of several days, the disease has appeared in this individual, he has been removed at once from his new abode, which has subsequently continued free, while the disorder has spread in the locality to which he has migrated. It is admitted that from 1st of September to 12th October, 158 patients came from English and Irish ports, and it is certain that the disease existed at that very time in the city, and in parts of the county of Cork, and most probably elsewhere also. To the supposed propagation of the distemper from one person to another in the ship, it is objected, first—that none of the medical men or nurses were attacked; but how far this immunity ought to be attributed to hygienic regulations, may be inferred from the following statement. During the last year’s epidemic here, of the attendants on 118 patients, twenty-five took the disease, nine fatally, six severely, and ten mildly. In the year 1832, in one town (Sligo) no fewer than eight medical men fell victims. *It is objected, secondly*,—that “the disease

was not propagated by the persons leaving the Dreadnought.” But as the persons are discharged in a state of health, this only militates against the conveyance of the disease by fomites which is altogether a separate question, and one from which, even if decided in the negative, we could not argue dogmatically against the personal communication of it. Some of the difficulties of contagion are thus enumerated: *first*—a person takes it in a place where disease, though previously existing, was so only in a trifling degree. But surely this is not valid. Why may not this person be one of the few? And besides we should receive with extreme caution accounts on this head, in a mercantile country, from the well known reluctance of such to acknowledge the existence of a pestilence. I well remember the scrupulous silence maintained by the Cork papers, respecting the prevalence of cholera here, while they ostentatiously related the occurrence of five cases of it in some part of our Indian territories! *Second objection*—“he is not shewn to have had any communication with the infected person.” But would we conclude that any particular case of fever in which the same could not be shewn, was therefore not infectious. *Third objection*—“he carries it about him so long a time without communicating the disease to others.” In reply, I would first refer to cases one and three, above, and in the next place, observe that a person during the period of incubation in hydrophobia, one frequently so much longer than that objected to in the Dantzic case, could not communicate the disease to another. *Fourth objection*—“he goes to another ship, and gives the disease to others.” Certainly, but not till the disease has been developed in himself. *Fifth objection*—“the contagion stops short, suddenly, and when every thing is favourable to its propagation, is not propagated.” Now this makes only against the idea of its spreading by infection alone, which I do not, and I believe very few do support. *Sixth objection*—“the short duration of the epidemic militates strongly against a long period of incubation.” To instance again the case of hydrophobia; suppose a number of people

bitten by a rabid animal; suppose each of these, with one exception, to manifest the disease at the average interval of twenty, forty days; suppose that after these no new case occurred for twelve months, the extreme period of incubation, and that at the end of this time the above single case becomes developed; would we be warranted in arguing, from the long period of incubation in this case, and the short duration of the preceding outbreak, that this single case was not the result of inoculation? And is it not the same thing to assert that, because the first case of cholera had a period of incubation of thirty days, and the second and third cases occurred on the following nights, these latter could not have been propagated by contagion? Does it follow that the period of incubation must be of the same duration in all cases? or, if a person bearing about him the morbid impression, escape the development of the disorder during thirty days, from certain circumstances of which we cannot be cognizant, (one of them, probably, being his removal from the place where the morbid influence was in a state of activity,) does it follow that another who receives the morbid impression in a place where he remains among the sick, must enjoy an equally protracted indemnity? Does analogy shew it? Is it proved that in fever, the period of incubation is in all cases the same? It is admitted, that the disease could not be attributed solely to any general atmospheric influences, while no obvious local cause could be assigned for it, either in the crowding or ventilation of the ship, the diet of the patients, the condition of the hold, or the previous medical constitution of the vessel. Infection is disallowed. What then is the "*tertium quid*?" When it is conceded that the immunity of the *Iphigenia* in 1832, and on the late occasion, could be attributed to the complete separation from the sick, does not this look like a recognition of infection? The tests to which a disease, about whose infectious or non-infectious character a dispute has arisen, is brought, are generally the following. *First*—Has it been shewn, that persons affected with the disease in question, have

arrived in a previously healthy place, and that the disease has soon after spread in that place? *Secondly*—Has the disease extended in proportion to the intercourse between the healthy and unhealthy? *Thirdly*—Has such disease prevailed more among persons who devoted themselves to its alleviation as medical men, nurses, and the friends of the sick? *Fourthly*—Is there any other cause to which the disease may reasonably be attributed? *Fifthly*—Has immunity been obtained by seclusion, and avoiding communication with the sick? *Sixthly*—Have the healthy become affected with the malady soon after proximity to, or contact with a person labouring under a similar one? I cannot help thinking, that viewed under these several lights the evidence preponderates on the side of infection. I cannot, neither do I wish to deny, that there are difficulties in the infectious hypothesis, but would merely observe, that not only on this but on every other point connected with physical science, scepticism can easily propose many more questions than philosophy can answer.

ART. III.—*On the protective Power of Vaccination.* By
HENRY B. EVANSON M. B., Physician to the Foundling
Hospital, Cork.

Two periods of twenty years have nearly passed away since Jenner's great discovery began to be adopted amongst us. The hesitation with which so great a novelty may have been received by rational minds, when first presented to them, quickly yielded to the proof by which it was sustained. The groundless fears of fanciful objectors soon ceased even to furnish matter for derision. The rare, invincible opponent was awed into silence, and vaccination, in a very short time after it was laid before the world by Dr. Jenner, being embraced every where as a deliverer from a dreadful scourge, ceased to encounter opposition. Notwithstanding the inestimable value of such a discovery, much carelessness has marked its use

in the upper classes, and in the lower, ignorance, apathy, prejudice, and superstition have prevented it even yet from becoming universal. Small-pox, propagated amongst the latter by itinerant inoculators, and communicated likewise by other means, unnoticed or unknown, becoming from time to time epidemic, involved many of the vaccinated in its contagion, producing, in several, anomalous febrile eruptions more or less resembling its own characters, and in some genuine variola. Towards the close of the first period, in 1817, 18, and 19, those cases had become so numerous, that in society at large not a little anxiety began to be felt, lest the confidence with which cow-pock had been regarded as a protector against a horrible disease, should ultimately prove fallacious; and the attention of medical men, directed by no less anxious feelings, being early drawn to them, they furnished matter for much professional discussion, public and private. The characters of genuine small-pox could not be mistaken. On this point there could be but little difference of opinion; and the fact that it did sometimes occur after perfect vaccination was, on the lapse of a period capable of furnishing proof, sufficiently established. But the nature of those other and much more numerous cases resembling it, yet differing much from it and from each other, did not admit at once of such decisive evidence. The variance in medical opinions on this subject, and the want of accordance frequently observable in the descriptions given to the public by those who wrote on such diseases, then as well as previously, can be adequately accounted for, and is satisfactorily explained by the admission that the contagion of small-pox is capable of producing a great variety of eruptive attacks, according to the susceptibility of the individuals liable to be in any way influenced by it: commencing with red spots analogous to the earliest discolorations of variola, but soon disappearing, comprising all those eruptions to which the term *varicella* properly belongs, and ending in the pestilential disease itself. This view was, to its full extent, maintained by Dr. John Thompson, and partly by Mr. Cross

in his valuable work on the variolous epidemic which visited Norwich in 1819. It was the conclusion formed by myself, about that time, from my own observation alone, and it has been confirmed by subsequent experience. I believe it to be now generally received by the members of the medical profession.

Any thing like a correct estimate of the protecting power of cow-pock, either positively or in comparison with small-pox, could not be made without a previous decision on this question. That decision being formed, there did not appear to be any difficulty in coming to a conclusion on the main subject, and at the close of the first period of twenty years, the following propositions were considered to be established respecting cow-pock.

1. That in a great majority of the vaccinated, it is a perfect protection against small-pox.

2. That in a small minority, it is but a partial protection, leaving them susceptible of small-pox in some of the various grades of its modified forms.

3. That in a few individuals, it is no protection at all.

The extensive examination which these matters received, not only enabled us to arrive at the foregoing positions in relation to cow-pock, but also brought to light a similar condition in those who have passed through variola, either casually or by inoculation ; with this difference, however, that the minority partially susceptible is smaller, and the individuals entirely unprotected fewer, than in the case of cow-pock.

If these facts did not realize all that was hoped for, neither did they furnish ground for serious disappointment. The modified small-pox which occurs after cow-pock, is devoid of danger, and its occurrence unmodified is rare. During twenty years in which I have been a member of the medical profession, having had a large establishment under my medical care for thirteen years, I have not seen more than five cases of perfect post-vaccine variola, if so many. I have witnessed but one fatal case, and I can call to mind only two more, in this city,

of which I have heard. In one of the latter some doubt has been expressed respecting the genuineness of the vaccination. During the same period I attended a gentleman under genuine small-pox, which he had also early in life by inoculation. He had several children who had been vaccinated, and none of them took the disease from him. I visited likewise a servant man who was thickly covered with the eruption, modified, however, as the pustules were in different states of advancement. This man told me he had had small-pox naturally. And in my Registry at the Foundling Hospital I have recorded one case, in which the character of the cicatrix in the arm of a child attacked by the disease, corresponded with that left by inoculation. I am not without suspicion that I might count another instance of this kind which I put down hastily as a case of imperfect vaccination. In neither of these last was the disorder at all modified.

Precision of language belongs to correct reasoning: the term secondary small-pox, implies its recurrence, and should be confined to that meaning; when it follows vaccination, that is adequately expressed by the epithet post-vaccine, whether modified or otherwise; and the name varicella (*variola varicella*) may still be used to designate its incomplete forms occurring before either.

The modified post-vaccine *variola* being a mild disease, and its modifications constituting an overwhelming majority, it follows, that the mortality is exceedingly small. The physician of the Small-pox Hospital of London has recently reported it to be seven per cent. on cases of *reputed* vaccination. I can well believe, that the mortality of small pox in that hospital is twenty-seven per cent. (it has been forty-one) for that accords with the nature of the disease, and is not contradicted by general experience. The small register which I subjoin, excluding the post-vaccine cases, gives one death in seven. If there be any considerable mortality of true post-vaccine *variola* there recorded (it should not be estimated by that of reputed cases) there must be such qualifying circumstances as will entirely prevent the adoption or application of that ratio any

where else. Very mild cases would scarcely seek admission into that establishment, or into any such, and any disease exciting smart fever, especially of the eruptive kind, in the pauper population of a great city, would produce deaths not likely to occur elsewhere. When inoculation with small-pox was at its height, it was estimated that 36,000 perished by the disease annually in the United Kingdom. The population of the United Kingdom was not then what it is now, and would not now be what it is, admitting every other cause, but for vaccination. Men of renown, from Sesrostis to Napoleon, may contest the honour of having destroyed the greatest number of human lives, and in vain might an assembled world labour to adjudicate; to the question, who saved the greatest number, there is but one answer, Jenner. I estimate the mortality of post-vaccine variola to be about equal to that directly following inoculation with small-pox. The multitudes of the perfectly protected are, therefore, in one sense or other, so much clear gain. They occupy the place not only of persons who would perish by casual small-pox, and of those preserved by inoculation, but, adding the partially protected, of those who might recover from a disease which sweeps away from one-sixth to one-half of the unprotected whom it invades, and inflicts suffering on all, and deformity on many, what an immense disproportion between the mortality of casual small-pox and that of inoculation! There is a much greater disproportion between the latter, and that to which all the vaccinated are liable. It has, too, been well observed, that variolous inoculation preserves the few, but sacrifices the many, so that the practice is not separable, even in our reasonings, from the horror of this pestilential disease.

The close of the second period of twenty years is now at hand, and the conclusions we formed at the termination of the first remain unshaken. No new facts have been developed, no information supplied, which would justify diminished confidence in cow-pock, or require any alteration of our opinions re-

specting it. An increase of post-vaccine variola (there has been no increase here) neither indicates a deterioration of the vaccine virus, as regards the many, nor a diminution of its protecting power, either gradually or at some definite age, as regards individuals. The vaccinated population of 1820 has been augmented by that of eighteen succeeding years. Supposing the same ratio of susceptibility and similar circumstances of exposure only, there should be a much greater number of variculous cases in the latter period than in the former. This increase ought to be expected for thirty years to come, (indeed longer, as vaccination is any thing but general amongst the poor,) for Jenner's discovery, practically speaking, is not yet forty years old, and it must reach the extreme term of human life, before it can include all ages. The allegations of an increase of post-vaccine variola are not founded upon data furnished by the United Kingdom, with the exception already alluded to, but by states, which, having the power of enforcing vaccination, had been more or less completely delivered from small-pox. Exactness of medical police would scarcely prevent a very general heedlessness, or want of attention, arising out of the general security, to the evidence which should be manifested of perfect vaccination in every individual case. One or two returns of the mortality in those instances give me a very strong impression that such has been more or less the fact. These data have also furnished occasion for putting forth an opinion ere now held and forgotten, that vaccination gives but a *temporary* protection. I know no analogy in the range of medical science which gives any support to such a notion. If there be any such analogy, I entirely overlook it. Whatever be the degree of protection afforded, it must be permanent. The escape of individuals after an exposure to contagion, and their being attacked on some other, at whatever interval, is perfectly reconcileable with this position, being in accordance with what we know of other communicable diseases; for the sum of the resistance at the one exposure being composed of the degree of

protection (an invariable quantity) and of the state of the constitution, together with external circumstances, (both continually varying,) we have in the latter an adequate cause of the different result. If a sufficient number of registers, carefully kept and correctly reported, should even establish as a fact, that a larger number of true post-vaccine cases occurs at some certain age than in any other, then various questions must be answered, before it can be justly accounted for—such as what lustrum, or definite period in human life, comprises the greatest number of the vaccinated in society, what age is most exposed to those varied conditions, internal and external, which furnish the fullest opportunity to receive the contagion of small-pox, and can alone prove complete exemptions. These and other questions must be answered, before we can infer a perfect anomaly. Calculations of this sort, even upon more certain data, are very liable to error. The injurious mistake of Dr. Watt ought not to be forgotten. Having examined the mortuary registers of Glasgow, he found that the ratio of deaths under the age of ten to the deaths in after life, increased instead of diminishing after the era of vaccination, and he came at once to the conclusion, that vaccination did not effect any ultimate saving of life; for that those preserved by it perished soon after by an increased virulence of other diseases, chiefly of measles. This error passed from one to another; arithmetic seemed to establish it, and it remained uncontradicted, until it was refuted by Mr. Robertson, of Manchester, who shewed that the ratio of the whole mortality to the population had been overlooked, and that there had indeed been a considerable saving of life.

A temporary protection, either for a definite period, or gradually diminishing with advance of years, is quite untenable. The probability of a deterioration in the protecting power of cow-pock, in the lapse of years, if transmitted unrenewed, through an indefinite series of human beings, is quite another matter. If the virulence of any contagious disease has abated in the course of time, (one instance at least will readily occur,)

how much more likely is that to take place, and how much sooner would it be developed in a disease so very easily interfered with as cow-pock ! That any such deterioration has yet taken place, there is no proof ; but that it will, soon or late, if not prevented, seems highly probable. Fortunately prevention is easy. The possibility of this deterioration, setting aside its probability, points out the expediency of a provision being made by the NATIONAL VACCINE INSTITUTION, for the renewal of the virus, within certain fixed periods. These periods may be arbitrarily chosen : they should not be short, because the virus must be carefully tested, neither should they be so long as to induce incautious persons to renew it for themselves. The managers of that Institution will, I hope, avail themselves of the example, if not of the assistance which Mr. Estlin, of Bristol, has laudably given them.

The register which I have kept at the Cork Foundling Hospital, of these diseases which occur but once in life, gives very satisfactory evidence of the protecting power of cow-pock. When measles appears within the walls, it spreads to every individual who may be liable to it. I know no instance of any child coming to the Infirmary with measles at one period, who had been in the house at its former visit, though one or two such instances might well occur. Small-pox is not less contagious than measles, and I am therefore justified in concluding, that those who escaped it within the walls, were not susceptible of it, except perhaps two or three in a very low degree. Judging from my experience in the Foundling Hospital, I would say, that these two diseases are equal as to contagious power. The following is the sum of

The Small-pox Register.

	SEX.	CASES.	DEATHS.	POST-VAC.	DEATHS.
1829.	Males, . .	12	1	„	„
	Females, . .	12	„	„	„
	Total, .	24	1	„	„
1831.	Males, . .	11	1	„	„
	Females, . .	17	2	1	„
	Total, .	28	3	1	„
1834.	Males, . .	32	5	„	„
	Females, . .	30	2	2	„
	Total, .	62	7	2	„
1836.	Males, . .	7	1	„	„
	Females, . .	11	1	3	„
	Total, .	18	2	3	„
1838-9.	Males, . .	13	3	5	„
	Females, . .	12	6	7	1
	Total, .	25	9	12	1

The result of the foregoing register is, that during five epidemics of small-pox there occurred only eighteen post-vaccine cases, of which one proved fatal. There are at present within the walls 240 vaccinated persons, including a few adults. To this number should be added all the vaccinated who passed through the house since 1829. The sum of these cannot now be ascertained. They must exceed 300, considerably, and may amount to 400; making the fullest allowance for an over estimate, and for the few who may have had post-vaccine variola whilst at nurse, the entire number of the vaccinated, amongst

whom these eighteen cases took place, is not less than 500. Of the eighteen, three only had perfect small-pox : one of the three, with two of the rest, had defective cicatrices. The fatal case seems to me to have been occasioned rather than caused by this disease. The patient died of sloughing of the mouth on the eighteenth day, giving every indication of recovery from small-pox apart from this, and leaving me in doubt, whether her death was the direct result of variola or was caused by a peculiar susceptibility towards mercury ; she had, however, taken no more than ten grains of calomel during the preceding part of her illness, combined with James's powder and rhubarb, and followed by purgatives. This child had been vaccinated by myself fourteen months previously, together with several others, and I observed on the eighth day, that the vesicles had been broken, on most of them, by their clothing. Of the remaining fifteen, two had merely papular eruptions, which disappeared on the third day : one of these, the son of the master of the house, was aged six years, and the other was an infant of one of the teachers ; a part of the rest had either a few pustules only, or a highly modified disease, and none an attack of any importance. They were all from the youngest children of the house, and the vaccination of these attacked by the last epidemic, was particularly recent. The progression in the post-vaccine column, 1, 2, 3, 12. may appear adverse to my assertion, that there has been no increase of post-vaccine variola in this quarter : that assertion I repeat with confidence. The augmentation of last summer is sufficiently explained by the mortality column of the same year. As the disease was unusually violent, so its contagious power was unusually great. There has been no increase of post-vaccine variola in this quarter since 1820. The most extensive vaccinator in this city is Dr. Kehoe, and his experience accords with mine. He informs me, too, that he never met a fatal case ; and that he has seen but one case where it was not modified. The fact that all those cases which I have recorded, occurred amongst the youngest and most recently vaccinated persons

subjected to the contagion of small-pox, at its five several visits in an establishment where all are nearly as one great family, is adverse to the notion that the protection conferred by vaccination diminishes with advance of years. As reasonably might it be argued from the data, whence that inference has been drawn, that the diminished, or last protection, is again required in the same individual, as that it suffers diminution; for *arithmetic* marks diminished numbers at both sides of the maximum of post-vaccine cases there recorded.

The numbers within the walls of the Foundling Hospital have varied from under 400 to 500, or even more, including the families of the teachers, and resident officers. The age of the children ranged from eight to puberty, until very lately, when several have been drafted in, not beneficially, so young as six. Formerly the infants were vaccinated before being giving out to nurse. Finding that a large number of them did not receive the cow-pock, and that the detention in the house, even for a few days, was injurious, I had them sent out as soon as possible after their reception, by which some saving of life was effected, and I recommended the governors to order a small premium to be paid the nurses for getting them vaccinated abroad. The advice was adopted, and, if carried fully into effect, tended to be as beneficial to the families of the nurses, as to the children of the Institution; but, I regret to say, it has been only in part acted on, the physician not having the power to enforce it, and the premium being very small. In November last, 105 children were drafted into the house from nurse. Of these I found forty without any mark of cow-pock or small-pox, of whom about half must have had the latter, as they have resisted its contagion since in the house, as well as vaccination. Of the rest, several were pitted by it. Adding to this fact, that not one-half of those now within the walls have vaccine marks, we thereby see at once how regardless the poor of this quarter are of the benefits of vaccination.

I was, for a short time, under the impression that I had got

possession of a fact, arising out of the vaccination of one of these forty children, which decided an interesting question concerning cow-pock. A child was brought up to the Infirmary on the fifth day of vaccination, with a papular eruption, similar to that of small-pox, three of the papulæ being in the two clusters of scars, which I had made with my lancet. This eruption went forward precisely as small-pox slightly modified, there being no difference between the sores of incision and those elsewhere. The difference between its characters and perfect variola was, that the pustules were in the early stage rather more shrunk, and that after maturation they became more elevated and globular, and recovery took place almost suddenly, when they began to discharge their contents, exactly as in the lowest degree of modification. This was unquestionably a case of modified small-pox.

Two children who escaped me, in the confusion arising out of drafting in an unusually large number, took the perfect disease afterwards. My first impression was, that it was directly produced by the vaccine virus, which I had inserted into the child's arm, from another who had a genuine vesicle, by matter procured from Dublin; but further consideration pointed out the probability that it was caused by the contagion of small-pox which had lurked in the establishment since the summer. The apparent coalition, however, between the two diseases remains still to be accounted for. In truth, the opinion that there is a remote identity in essence, between small-pox and cow-pock, is something more than conjectural. We know several diseases which destroy the susceptibility of the constitution for a recurrence of the same. We know of but the one instance in which a reciprocal agency is thus exerted between two diseases, supposed to be different. The question, then, not illogically arises, may they not be essentially the same? There is, moreover, a resemblance between the vaccine vesicle and the small-pox pustule at a certain stage. There is the indented centre, the elevated and rounded margin, the matter more or less diaphanous. The similarity of anatomical structure, supplies another

point, though a variance be found, in this respect, between perfect small-pox and some of its modified forms. Were this identity ever established, it would indicate that small-pox had its origin in some of the lower animals.

The large number of small-pox cases, which occurred in 1834, arose from its having been brought in by one of the children just drafted in, and it spread before I could protect them. Three of those whom I vaccinated at that time, had small-pox and cow-pock concurrently, without modification of the former.

How greatly it is to be lamented, that an object so important as the prevention of the ravages of small-pox, has never been effectively brought under the consideration of our legislature! There are two modes of proceeding conjointly necessary in order to be effectual; one is, the organizing vaccine institutions, and the other is, making the propagation of small-pox by inoculation penal, and making provision for insulating strolling beggars, who carry it from place to place. How culpable is the legislature which, with a single exception, has entirely neglected these matters. Last summer a beggar-woman took her place not forty yards from my door, having in her arms a child dying of the small-pox; on being threatened by the police, she removed to a less public place, but that was all. There should have been legislative provision for withdrawing this woman from public contact, and for disinfecting her before she were released. My office as Physician to the Foundling Hospital has brought before me another mode in which small-pox is propagated throughout the land. Itinerant inoculators, availing themselves of the ignorance and prejudices of their fellows, for a small fee communicated the disease from one to another. Some of their tickets have been brought me by foundling nurses, who mistook the instructions they received about vaccinating the children. One of these I recollect, ran thus, "*I scratify that I have ejaculated,*" &c. &c. Among the children at present within the walls of the hospital, there are about a dozen, whose arms attest the fact of such *ejaculation*.

A penalty of five pounds, or imprisonment in the common jail for not less than one month, upon summary conviction before magistrates at petty sessions, would suffice to arrest the propagation of pestilence in this mode. I regret to say that the fine is needed as well as incarceration, in order to meet a *higher class* than the itinerant inoculators. The only exception which should be made in a law of this kind is, in those cases where small-pox may have actually appeared in the household previously to the inoculation.

Re-vaccination has been recommended, as a practical deduction from some of the opinions or notions which have been combated in the foregoing paper. Re-vaccination of the poor generally would be unwise, because the utmost that their improvident habits will ever submit to is, to receive vaccination once, and the effort to repeat it would only, in their minds, throw discredit on the whole matter, and thus lead to limit it in the first instance. Amongst the higher classes re-vaccination is no novelty. Good sense being the guide as to the mode, I think the repetition wise and to be commended.

The Foundling Hospital of Cork, being superseded by the general provision for the destitute, just coming into operation, I avail myself of this opportunity to state, that the gross proportional mortality for a period of fourteen years, (which ought to yield a maximum,) as returned by myself to the Sub-Commissioners of Inquiry into the state of the Irish Poor, was under forty-nine per cent. A low rate which were impossible amongst deserted children, if the reception of them had been confined to the first year of life.

ART. IV.—*Cases of transverse Presentations, and one of Detachment of the Os Uteri, with Observations, being the Substance of a Clinical Lecture delivered at the Coombe Lying-in Hospital, on Saturday the 29th of June, 1839.*
By Robert F. Power, Member of the Royal College of Surgeons in Ireland and one of the Surgeons to the Coombe Lying-in Hospital, &c.

THE majority of cases which come under the observation of the obstetric practitioner, require in their management but little skill or interference ; and hence, unfortunately, an opinion prevails that a knowledge of midwifery is easily acquired, and is of but secondary importance. It would be quite unnecessary for me to dwell upon the mischievous and dangerous tendencies of such an opinion ; daily experience shews the many untoward circumstances that will occur even in the simplest cases, not to mention those anomalous ones, in which the life of either the mother or the child, or of both is endangered ; or, more appalling still, when one must, of necessity, be sacrificed to afford a chance of safety to the other. In such instances the greatest dexterity, guided by sound practical information, is required to ensure a successful termination ; and as a man may not know the instant, when he shall be called upon to treat cases of this description, he should be always prepared for the emergency. Within the last few weeks there have occurred, amongst the patients of this hospital, some of those *preternatural cases*, and it is the object of the present lecture to direct your attention to two or three of the most important. *Preternatural labours* are generally divided into two orders ; in the *first* are included those cases in which the *breech* or any part of the *lower extremities* presents, and in the *second* are enumerated those in which the child lies *transversely*. It is of the utmost importance that we should discriminate between these cases at an early stage of labour, when we have the opportunity of then making an examination ; because, in the first order of cases the efforts of

nature alone are, in general, able to effect the delivery; whereas in the other we are almost always called upon to interfere; and the success of our interference is mainly dependent upon the period at which it is employed. It is to the latter class, or rather to that division of it which include presentations of the *shoulder and arm*, that the cases which are the subject of this day's lecture belong. In three of the cases before me, the shoulder and arm were the presenting parts, and they are interesting, inasmuch as they exemplify the different modes in which delivery may be accomplished under such circumstances; thus, in one I effected it by *turning*, in another I was compelled to have recourse to *embryotomy*, while in a third, a process, which is termed *spontaneous evolution*, took place. Before I recall your memory to the first of these cases, it would, perhaps, be useful to make a few general remarks upon this species of presentation. As I mentioned before, and as the details of this case will shew you, it is of the greatest importance to ascertain correctly the nature of the presentation in an early period of labour; otherwise you may lose the most favourable opportunity for relieving your patient when it could be done with more ease to yourself, and what is more important still, with much less danger to the mother or her child. In most cases, however, until labour is somewhat advanced in this stage, we cannot accurately determine the precise nature of the presentation, but there are symptoms the presence of which will lead us to suspect a deviation from the natural course. If, for example, we find the os uteri to dilate slowly, although flaccid, and that the membranes protrude through it in an elongated pouch, like the finger of a glove, in place of the rounded form which it presents in natural cases; and if in addition we are unable to detect any part of the child, although the finger is passed up to its fullest extent, we will have tolerably well grounded fears that a transverse position of the child prevents its descending into the pelvis. In such a case we should use the utmost vigilance and caution, watching the progress of the labour, until it arrived sufficiently far to enable us to detect

with accuracy the presenting part, and take advantage of the most favourable opportunity for employing our remedial measures. But it is by no means easy in all cases, even when the presenting part is within the range of the finger, to determine accurately what that part is. The *characteristic signs* of a shoulder presentation are, first, the pointed *acromion* being most dependent, and meeting your finger, guided by it, the latter will glide along the spine of the scapula *posteriorly*, and may reach and feel the spinous processes of the *vertebræ*; while *anteriorly* it will detect the clavicle and ribs, and enter the cavity of the axilla without being obstructed by any parts resembling the genitals or anus. There is little or no danger of our confounding this presentation with a head, but the distinguishing marks between it and the *breech* are not always clearly defined. The soft feel which the top of the shoulder sometimes communicates to the touch, has led to its being mistaken for one of the nates; a little care in the examination will, however, soon clear up the doubt, *the differential signs* being the attachment of the arm, the more prominent and pointed shoulder, and the absence of the anus and genital organs, &c. The presence of the hand in the vagina, although a presumptive, is by no means a *positive* proof that the shoulder is the presenting part, for it may sometimes descend by the side of a head or breech. In such a case it is of great moment to ascertain accurately the part above. In making the examination, however, you should be careful lest you rupture the membranes, particularly as the presentation may be so high up, as to be out of reach of the finger, and require the introduction of the greater part or the whole of the hand into the vagina, before it can be satisfactorily ascertained. In some instances the symptoms of shoulder presentations are very obscure, and it requires considerable tact and application to make them out. Here your diagnosis must be assisted by evidence drawn from *negative* as well as *positive* indications, weighing the absence of certain appearances with the presence of others, and forming an opinion from conclusions thus obtained, after patient and cautious investigation.

Having determined that the shoulder is the part presenting, your next point is to ascertain the period of gestation; and, if possible whether the child is living or not. If the patient is only at, or a little beyond the sixth month, the child dead, and the pelvis roomy, there may be a chance of its being expelled by being doubled upon itself, and thus forced through the pelvic passages; but, generally speaking, if gestation have advanced beyond this period one of three things must occur, either the patient will die exhausted, rupture of the uterus take place, or the position of the child must be changed by the hand: in other words, the operation of *turning* must be performed. And now the question arises at what period of labour will you adopt that proceeding? There are two periods at which you may be called upon to treat a case of this description; one, is while the membranes are intact, the other after they have given way and the waters have escaped. Where you have the option, the former is always preferable and should be selected. When the process of dilatation has not proceeded sufficiently far, either in the external parts, in the vagina, or os uteri, to admit the introduction of the hand, you must wait for their relaxation as long as you can, with safety to the bag of membranes. But if the os has attained the size of a half crown piece, be flaccid and dilatable, you may then proceed. The chief difficulty in this stage is the dilatation of the vagina and mouth of the womb, and this is always greatest in premature cases; arising probably from the imperfectly developed state of the cervix uteri. While the uterus is distended with the liquor amnii the operation is easy to a steady and experienced practitioner, but when unfortunately the case is not seen or attended to until after the waters have been discharged and the parietes of the uterus have closed around the child, it then becomes one of danger and difficulty, and these are increased in proportion as the second stage of the labour progresses. The uterine contractions diminish the volume and bring the inner surface of the viscus into close opposition with the body of the

child, and the greatest difficulty is experienced in consequence of the expulsive efforts of that organ resisting the advance of the hand, or at a later period, preventing its entrance altogether, by blocking up the pelvis with the impacted foetus. The cases of Mary Ryland and Catherine Barnwell exemplify these two conditions, the former of which I will now read for you as it has been given to me by Mr. Banon, our resident.

“Mary Ryland, aged 36, was admitted into the hospital on fourth of April, at four o’clock P. M. States that she is two years married, and has had three children, all natural births, and living. Her present labour commenced on the previous day by slight pains. Having no one to mind her little family at home, she delayed leaving them until the last moment, and did not quit her room for hospital until after the membranes had burst, which took place about two hours previous to her admission. On examination, the left arm was found low down in the vagina and a loop of funis accompanying it, the pulsation in which was still present but very feeble. Mr. Power was immediately sent for, and shortly afterwards performed the operation of turning, and delivered the woman of a full grown female child, which was dead, and had a preternaturally large head, in which the bones were much separated. The placenta came away naturally, and the woman recovered without any bad symptom, and left the hospital in five days.”

When I saw this case, as its recital informs you, the waters had come away, and the left arm, together with the funis, was protruded through the external parts. An examination *per vaginam* discovered the left shoulder occupying the brim of the pelvis, in what the French call the second position. The funis had ceased to pulsate, and the patient seemed much exhausted. As there was no time to be lost, I determined, with the concurrence of my colleague, Mr. Carmichael, to proceed at once to turn the child. A mild restorative was administered, in consequence of the weakened state of the patient, and her bladder having been emptied, she was placed on her left side in the

usual obstetric position, with the buttocks brought a little over the edge of the couch. Having taken off my coat, and anointed the back of my left hand and arm up to the elbow, I knelt down beside her and proceeded to introduce my hand, in a conical form, into the vagina. I now found that the presence of the child's arm here interfered with the passage of mine, and judging from the extent to which it was then extruded, that the shoulder and a portion of the side had descended lower into the brim, and would not only obstruct the introduction of the hand, but, by their lodgment there, would prevent the descent of the breech, I resolved to attempt their return; and, taking advantage of an interval between the pains, I succeeded in gently pushing upwards the descending parts, using the protruded arm as a lever, as has been recommended by Dr. Ramsbotham, and following up this movement steadily and quietly, I succeeded in getting my hand into the uterus beneath the body of the child.

This is a stage of the operation, that in a case like the present, requires great caution; for, if unmindful of the axes of the pelvis, you may thrust your hand through the vagina, or if on entering the uterus, you force on during its action, you may do most serious injury to its structure, and perhaps cause the destruction of your patient. Having thus got my hand within the uterus, I placed it flat upon the body of the child, and taking advantage of each interval of quiet between the contractions, I gently slid it along, until I reached a foot, which I seized between the sides of my fingers, and brought down; after which the delivery was finished in the usual way. You may recollect I had some delay in extracting the head, owing to its enormous size, the bones being separated nearly as much as in a hydrocephalic fœtus. In this case the mouth of the womb was dilated sufficiently to admit the passage of my hand; but the difficulty to be encountered was, in the violent expulsive efforts of the uterus, which pressed closely upon the body of the child. These, however, I was in a great measure enabled to avoid, by

taking advantage of the important fact of the progressive contractions of the uterus, as explained by Mr. Hugh Carmichael. The child was placed across the pelvis, with the head resting on the right iliac fossa, and the feet above and to the left. (This drawing will shew you its relative position.) Under these circumstances, I used my left hand, which indeed I generally prefer, and which in the present instance offered me two important advantages; first, I was enabled to pass beneath and behind the body of the child, (which lay with its anterior surface looking downwards and a little backwards,) and thus to get into the space formed by the curvature of the trunk, where laying my hand flat upon the abdomen, I could glide it along that part directly towards the feet; and secondly, my hand was first brought in apposition with the *posterior wall of the uterus*, which is the last part of that organ to take on the *contractile action*,* and by which I avoided not only the resistance which the expulsive efforts of the uterus offer, but also the chance of injury which the parietes may receive from their forcible contractions upon the hard knuckles of the operator. Three different modes have been recommended for the performance of this operation. By the first you are advised to introduce your hand into the uterus, grasp the head, bring it to the brim of the pelvis, and convert the case into a natural presentation. This mode would be the safest to the child, inasmuch as it would lessen the chance of pressure on the funis; but it would be by far the most difficult to the operator, and consequently the most dangerous to the mother, from the risk of lacerations and contusions of the uterus and vagina being greater. It is therefore almost altogether abandoned in these countries, but is, I believe

* See Mr. Hugh Carmichael's Paper in the Dublin Journal for January, 1839. In the present instance I had the gratification of calling my colleague's attention, as he stood beside me during the operation, to this practical illustration of the truth of his position, and I may here add that every day's experience confirms me in the belief of the general correctness of his views.

still occasionally performed in France. The second mode which was recommended in these cases was, that of seizing the breech, and by bringing it into the pelvis, convert the presentation into the next most favourable to the head. But this also being attended with difficulties nearly as embarrassing as the other, was abandoned in favour of the third, that now generally adopted as the safest and most practicable, namely, delivering a child by bringing down *a foot or a knee*. In many of the works upon midwifery you are recommended to bring down both feet at the same time ; this I do not consider advisable, and for these reasons : you expose the umbilical cord to an injurious and generally fatal pressure, the most constant cause of the death of the child : the vaginal canal will not be sufficiently dilated to allow of the quick passage of the head ; and again, in the event of there being twins, you run the risk of bringing down a foot of each child, a circumstance which I need not tell you would be attended with the most distressing and serious consequences. On the contrary, by taking down only one foot, you prepare the parts by sufficient dilatation for the speedy completion of the labour ; a sulcus will be formed between the body of the child and the other limb, in which the funis will lie protected from the pressure of the surrounding parts during its transit, and thus you gain by this method some of the advantages of a breech presentation. My friend Dr. Breen of this city, recommends the bringing down of one of the knees, particularly in cases where the feet are high up.* If you ascertain correctly the position of the child, you will then know exactly in what direction to seek the feet, and in a case where the waters have been evacuated, and the walls of the uterus have closed upon the foetal body, it is absolutely essential that you should know precisely for what point to make ; in these instances you cannot grope about for them in the womb, you may

* Edinburgh Med. and Surg. Journal, vol. xiv. p. 29.

perhaps, have barely room to slide your flattened hand along the child's body during the periods of uterine quiescence, and unless you are sure as to the right direction to give it, you had better not attempt the operation at all. It is not always an easy matter to decide this satisfactorily, but in my opinion a careful examiner will in all cases be able to define the relative position of the parts. Where the hand protrudes some diagnostic rules have been laid down. We can readily distinguish the right from the left hand, by placing it in opposition to our own, when the thumbs will be found opposed, and knowing that the palm of the child's hand will look in the same direction as its abdomen, unless it is twisted, we can make a sure guess from its aspect, as to the position of the child. Four positions have been described, two for the right shoulder, and two for the left. The protruded palm may serve as an index to each of these; for example:—if the palm of the right hand is looking forwards, the head will be to the right, with the face anterior; if, on the contrary, the palm looks backwards, the head will be to the left, and the face will look towards the spine. The same rule holds good with respect to the left hand; if it be the protruding member, and that its palm be directed forwards, the head will occupy the left iliac region with the face directed forwards, and *vice versa*. Sometimes the arm is doubled, and the elbow descends into the vagina, or the arm may not appear, the shoulder alone being impacted in the brim. In the former case, when there is any doubt, it has been recommended to gently hook down the forearm and decide it; but in the latter, the sense of touch, determining the peculiarity of each region within the range of your fingers, must alone enable you to decide. But is version practicable in all cases of shoulder presentation? Unfortunately the next case I will read for you answers in the negative. Here the labour had advanced too far before aid was sought, and in consequence another line of treatment was required.

Catherine Barnwall, æt. 31, living at No. 6, Coombe, sent to the hospital on Wednesday evening the 29th of May. When visited she stated that she had been attacked with pains the morning before, but that as she had not come to her full time, she hoped it was not her labour, and therefore did not then apply for any assistance. Mr. M'Guire, the gentleman on duty, found the arm protruding through the external parts, and immediately sent for me. Unfortunately, I was from home when the messenger called, and did not see the patient until half-past six o'clock in the evening. When I then saw her, she was very restless, rolling from side to side, her countenance expressing great anxiety, and the uterus contracting with considerable force and frequency. She was literally naked, and lying only on a little straw upon a wretched settle. The left arm of the child, livid and desquamated, was lying completely outside the external parts, which were very tumid and painful. With some difficulty I got my hand into the vagina, sufficiently far without risk to the maternal structures, to ascertain the state of the parts above. The left shoulder and part of the chest were jammed in the brim of the pelvis, into which it had descended a little; the child's head was to the left and a little anterior, while the breech and lower extremities were to the right and a little posterior. The anterior part of its body looked forwards and slightly downwards. On examining the abdomen I found the uterine tumour sunk in the pelvis and very hard. The patient stated that this was her seventh labour, and of the other six, five of her children were still-born, one of which she termed a *cross birth*, and of two others she was delivered by instruments. All her labours were difficult. In the present instance she thinks she is a little more than seven months gone, and suspects her child to be dead about two days. From the state of the parts I deemed turning, by introducing the hand into the womb, impracticable, but considering the position of the child and its small size, I calculated that if I could succeed in elevating the shoulder I would be able to hook the breech into the pelvis, when

nature would speedily effect the rest. I attempted this with the utmost caution and gentleness during the absence of a pain, but I found it impossible to move the constricted parts, and fearing to use any force I desisted from the attempt. Delay now only added to the dangers and difficulties of the case. The previous history and small size of the pelvis gave me no hope that the expulsion of the child would be effected by the unaided efforts of the uterus, while its strong and continued action occasioned fears for its integrity. The death of the child being beyond doubt, the indication was to relieve the mother by the easiest and most expeditious method, which in the present instance consisted in lessening the bulk of the foetal trunk. This I effected by perforating the chest and having compressed the ribs beneath the integuments so as to prevent their broken extremities injuring the maternal structures in their transit, I succeeded in getting sufficient hold on the doubled body to enable me to bring the breech into the cavity of the pelvis; the shoulder ascending as it descended. Very soon after it was expelled the posterior surfaces of the lower extremities, sweeping over the perineum, while the forehead of the child revolved along the smooth planes of the ilium and ischium, into the hollow of the sacrum, and the delivery was completed without further trouble. The woman recovered perfectly in few days. During the operation I had recourse to an expedient now very much if not altogether exploded, and decried as unnecessary, namely, the amputation of the protruded limb. As a general rule it may be certainly objectionable, but in the details of each case you must be guided by the peculiar circumstances that may arise, and the present is an illustration of this. In consequence of the swollen and undilatable state of the external parts, I found considerable difficulty in getting in my hand sufficiently far to enable me to effect the process just described; and this difficulty was decidedly increased by the protruded arm which blocked up the already too narrow vagina. The child's death being certain, and the case being one for *evisceration*, and not

for *turning*, I did not hesitate an instant in removing the obstructing member, which I amputated at the humeral epiphysis, and I found that its removal greatly facilitated my manipulations in the vagina. Thus, when authors tell you it is always unnecessary, I say the exigencies of a case may render it expedient ; and I would not hesitate adopting the same plan tomorrow under similar circumstances. Of course I only allude to cases requiring evisceration, and where the death of the foetus is beyond doubt.

The strong action of the uterus being the chief source of embarrassment and danger in these cases, various means have been employed for the purpose of quieting it. Many authors recommend the abstraction of blood from the patient, not only with a view of reducing the uterine action, but as a preventative of after inflammation. Large doses of opium have also been given, and a solution of tartarized antimony in conjunction with opium, is very deservedly recommended as useful in promoting relaxation. Fomentations to the vulva, the warm bath, and even the tobacco enema, have been advised with the same intent. I must own that in my opinion, in the majority of cases, more is lost by the delay occasioned in the exhibition of those remedies than is gained by their adoption ; and sometimes, in waiting for their action, the precious moments are allowed to escape when we could interfere with greater certainty and ease. If, too, the uterus be reduced in size by permanent tonic contractions, they will be unavailing. There is another point that should not be overlooked, and that is their after effects. If the powers of the patient be reduced to a low ebb, we destroy the means of her sustaining the very actions that are afterwards essential to her recovery. "If," says Dr. Ramsbotham, "we deliver the patient either while under syncope from bleeding, or stupefaction from opium or tobacco, we should be emptying the uterus at a time when it could not exert its contractile energies : we should consequently leave it in a flaccid state, and bring the patient into the greatest peril." The experience I derive from the practice of

this hospital induces me to coincide with Dr. Ramsbotham; and the relaxed habits of most of the patients that apply here require great caution to be observed in the use of depletion, particularly with the lancet. Cases of uterine rigidity, or of violent action of this organ in plethoric females, will sometimes occur, when the exhibition of relaxing and sedative agents will be of essential service. Of these I prefer (with the aid of the lancet, if necessary) the tartar emetic solution, in combination with the acetum opii, as recommended by Dr. Collins.

Inflammation is one of the after consequences most dreaded in cases of this description, whether induced by the hand of the operator, or by injury sustained by the uterus itself. With the view of guarding, or more properly speaking, preparing my patient against such a misfortune, I am in the habit of prescribing, after the operation, four or five grains of calomel with a grain of the watery extract of opium; and if at night, to be followed in the morning by a draught containing castor oil, and spirits of turpentine. If the abdomen continues tender, I prefer the application of a large linseed meal poultice over it, to stuping. It is more agreeable and less teasing to the patient, and fulfils the indication better. The state of the vulva and vagina should claim particular attention; the strictest cleanliness should be enjoined, and for that purpose laving the vagina with warm water by means of the elastic bottle, will be found both grateful and useful. When the lochial discharge is scanty or altogether suppressed, the application of the mustard cataplasm to the groins, as recommended by Mr. Hugh Carmichael,* is the most certain means, I know, of restoring the healthy condition of the uterus. If inflammation set in, you must of course treat it in the usual way, regulated, however, by the condition of the patient and the urgency of the case. These dangerous consequences would less frequently occur, if opera-

* See Dublin Medical Press, No. XXI. May 29th, 1839.

tors would bear in mind, what I implore of you never to forget, namely, the extreme delicacy of the structures in which you are acting, in all obstetric operations, for injury to them may be attended with results, that might be either immediately fatal, or might leave your patient afterwards to drag on a miserable existence, in a state of loathsome suffering, to which death would be a happy release.

In the cases just related, delivery was effected by art, but in the instance of malposition, to which I will now direct your attention, nature effected the relief by a process that has been termed *spontaneous evolution*. This occurred in an out-patient, named Jones, who lives in Rainsford-street. She had had eleven previous births, all natural, and in the present instance she believed herself to be in about the seventh month of pregnancy. Her labour commenced on the 6th of May, but she did not apply at the hospital until the next morning; and when the pupil on duty visited her, he observed the arm protruding, and immediately sent for me. On my arrival I found the woman had been delivered of a dead foetus, apparently between the sixth and seventh month. The lividity of the protruded arm, and the tumour on the chest and forepart of the right shoulder, left no doubt as to the nature of the presentation, and the manner in which delivery was effected was in accordance with the general routine of such cases. Upon examining the patient, I found that the pelvis was somewhat larger than the ordinary size, and on inquiry it was ascertained, that her former parturitions were quickly effected, and some of them attended with hæmorrhage. The delivery of the child by the natural efforts in transverse presentations is a very rare occurrence, and *generally speaking*, can only take place in premature cases, and where the pelvis is capacious. Since my appointment to this Hospital, a period now of nearly five years, and during which upwards of *ten thousand* women have been delivered under its auspices, but three cases of this nature occurred, and these were attended with circumstances nearly simi-

lar to the one just mentioned. Here the child is generally still-born, which is easily accounted for, when you reflect upon the violence which it receives in its doubled position while passing through the pelvis. Dr. F. H. Ramsbotham mentions having had seven cases, in three of which the children were born alive; in one instance of the seven, the child had arrived at the full period. The mechanism of this mode of parturition was supposed by Dr. Denman to consist in a complete evolution of the child, effected by the “uterus pressing down upon its inferior extremities, which are the only parts capable of being moved; they are forced gradually lower, making room as they are pressed down for the reception of some other part into the cavity of the uterus, which they have evacuated, till the body turning, as it were upon its own axis, the breech of the child is expelled as in an original presentation of that part.” This opinion of Denman was that generally received and taught as the true explanation; and the merit of pointing out its fallacy, as well as shewing how such a delivery was really effected, belongs to our fellow citizen, Dr. Douglas, who first published his views upon the subject in 1811.

According to his description, the shoulder and chest being propelled lower into the pelvic cavity by the continued and forcible action of the uterus, the arm is consequently protruded altogether externally, and the acromion points under the symphysis pubis; the uterine action continuing, the point of the shoulder ascends towards the mons veneris, thus making room in one side of the pelvis for the loins and breech, which descend into it, closely following the displaced parts. The breech thus gets into the hollow of the sacrum, and is thence expelled, sweeping over the floor of the perineum which is greatly distended. The head is still above the introit, and the point of the shoulder being external, the clavicle is strongly pressed against the under surface of the symphysis pubis, on which point the foetal body partially revolves as on an axis during this process. The other shoulder and arm then come

down, and the head is the last expelled. This is the true explanation of the process, the child passes in a doubled state; and to use the words of the Doctor, "it is incompatible with the received ideas of uterine action, to suppose, that the uterus, when contracting so powerfully as to force down that part of the child which was at its fundus, would at the same moment form a vacuum into which another portion, already low down in the pelvis, should recede."

One of the greatest dangers consequent upon transverse presentations is the liability of the uterus to *rupture*: fortunately it is an accident of rare occurrence, but nevertheless you should bear its possibility in mind, and be upon your guard, particularly if the uterine action be violent, and if there be any deformity of the pelvis. This lesion may take place in any part of the uterus, in the fundus, the body, the cervix, or the os. Its direction varies, and according to its extent, it may be divided into *partial* and *complete*. In the *first* the peritoneal coat alone may be frayed, while the parenchyma and lining membrane may be perfect, or the lining membrane may have given way, with a partial laceration of the middle or proper structure, or both of these may be rent while the peritoneum remains whole. In the second, the rupture extends through the entire substance of the organ. This formidable accident may occur in any order of presentation, caused either by violent uterine action in difficult labours, or in cases of pelvic malformation, or from abnormal softening or thinning of the parietes of the womb, predisposing them to laceration; or it may be produced by the hands of unskilful operators. It may happen to a young woman in her first labour, or, as is most frequent, to the mother who has borne many children. No region of the womb is exempt from this accident, but the situation in which it occurs greatly modifies its effects, thus lacerations of the fundus or body, even though *partial*, and particularly if the serous coat be engaged, are, I may say, invariably fatal, while rents, and even *complete detachment* of the os and a portion of the cervix, may

take place, and not be followed by any serious result. From this we are led to suppose that there is a difference in the organization of the parts, and to this difference, I think, may be referred the immunity which so often follows injuries confined to the os and neck. The serous membrane, for example, which envelopes the fundus and body, does not extend *on the uterus* beyond these regions; it is also less vascular, the chief branches of the uterine and spermatic arteries being distributed to the upper parts of the organ.* The larger venous trunks, in like manner, are best seen there,† and the enlargement which those vessels undergo during pregnancy is nearly altogether confined to those regions, cases of placenta presentations of course excepted. The nerves are also distributed more freely over the body and fundus. If the parenchyma of the uterus be attentively examined, it will be found at the os and adjoining neck, denser and paler than elsewhere.‡

Are there any conditions of the os in which a knowledge of the peculiarities just stated might influence the treatment? It sometimes happens that at the commencement of labour, the os, in place of taking on the natural and healthy action of dilatation, may be either completely closed, leaving no trace of an aperture, or it may be rigid, constricted, and undilatable. This state may either depend upon the os being surrounded by a structure entirely undilatable, but in which no evidence of organic change can be detected, or it may be caused by structural alterations, depending upon morbid depositions in or around the part; or perhaps the effects of injuries sustained in former labours, the results of abscesses, ulcerations, and cicatrizations may have changed its figure, and altered the relative situation of the adjoining parts. In some instances “a hard tumour, or a more malignant and active deposit may have imbedded itself in these parts, totally altering the os and the

* Tiedeman Tab. Arter.

† Breschet Système Vieneux, (Cloquet, Liv. 4.)

‡ Boyer, Trait D'Anat. tom. 4, p. 57.

natural structure of the cervix. In one essential particular, all these variations will be found to agree; viz. in the difficulty with which the os and the cervix are dilated; while in some and not a few of them, the susceptibility of dilatation will have been entirely destroyed." (Ashwell). In the simpler forms of these cases, the ordinary relaxants such as bleeding, tartar emetic, opium, &c. will sometimes succeed in producing dilatation, and therefore should always be used, unless contra-indicated by some particular features of the case, but in the severer and more complicated forms they will generally fail, and then two other means suggest themselves, viz. *artificial dilatation or incision*. The former is only admissible in the healthy state of the os, or where there is no structural disease that will prevent it yielding readily to the pressure of the hand, "but when the cervix is rigid, contracted and diseased, and the os so small as scarcely to be recognized, powerful and long continued artificial dilatation must be a dangerous remedy. It is scarcely expected that it should relax the parts and lead to dilatation: it is much more likely that it should irritate, and thus induce inflammation, gangrene, and death." (Ashwell). Other circumstances are to be taken into account, viz. the dangers of *delay*—exhaustion, with its consequent collapse—or rupture of the womb. In midwifery you can act by no fixed standard of time, and interference which might be safely delayed with one patient, might, with similar symptoms, be promptly called for with another. Here then the question arises, are there any strong anatomical and physiological causes to prevent the employment of incision in certain states of the os? I think not; and the result of those cases in which such an operation was timely performed proves its safety and utility. If the statement I have made relative to its organization, be correct, their incision is not likely to be attended with hæmorrhage, nor to occasion much, if indeed any pain, and such we find to be the facts in the cases reported.*

* Mr. Tweedie's and Dr. Ashwell's cases in the 8th No. of Guy's Hospital Reports.

I have been induced to make these remarks in consequence of the perusal of a very interesting paper from the pen of Dr. Ashwell, in the late number of Guy's Hospital Reports, in which the accomplished author clearly proves the advantage of incision in certain cases of rigidity; and also by a very extraordinary case of *complete detachment* of the os uteri, which occurred in the practice of my friend and colleague, Mr. Hugh Carmichael, and which he was kind enough to communicate to me. In fact some of the circumstances attendant upon this case so strongly illustrate the value of the practice recommended by the learned lecturer of Guy's, that I was induced to preface it by the foregoing observations. Mr. Carmichael's statement to me of it is as follows:

“Late one evening, in the course of the last autumn, I was requested to visit a young unmarried female, who, I was informed, was about to be confined of her first child. On my arrival I learned that about an hour previous to my being sent for, she was from home, and when at some distance from it, the waters, as it is termed, broke, and that before she could reach her residence they had been almost all discharged. On making an examination, I found the os uteri sufficiently dilated to admit the point of my finger, but thin and hard; the pains slight, but regular. She continued in this state the entire of the next day, the following night, and a part of the ensuing day; the pains at no time increasing beyond those of the first stage of parturition. During this period, though the pains were insufficient, nevertheless, the head progressed, the os dilating but very slowly, until the dilatation became about the size of a crown piece, beyond which it did not extend, its edges still continuing hard and rimmy. There was no deformity of the pelvis. Considering that the obstinacy which the os exhibited might probably result from the insufficiency of the pains, I determined on inducing them, if possible, to a certain extent, and with that view, on the second day, administered the *ergot of rye* in such doses as to throw the uterus rather upon the tension, than induce the strong

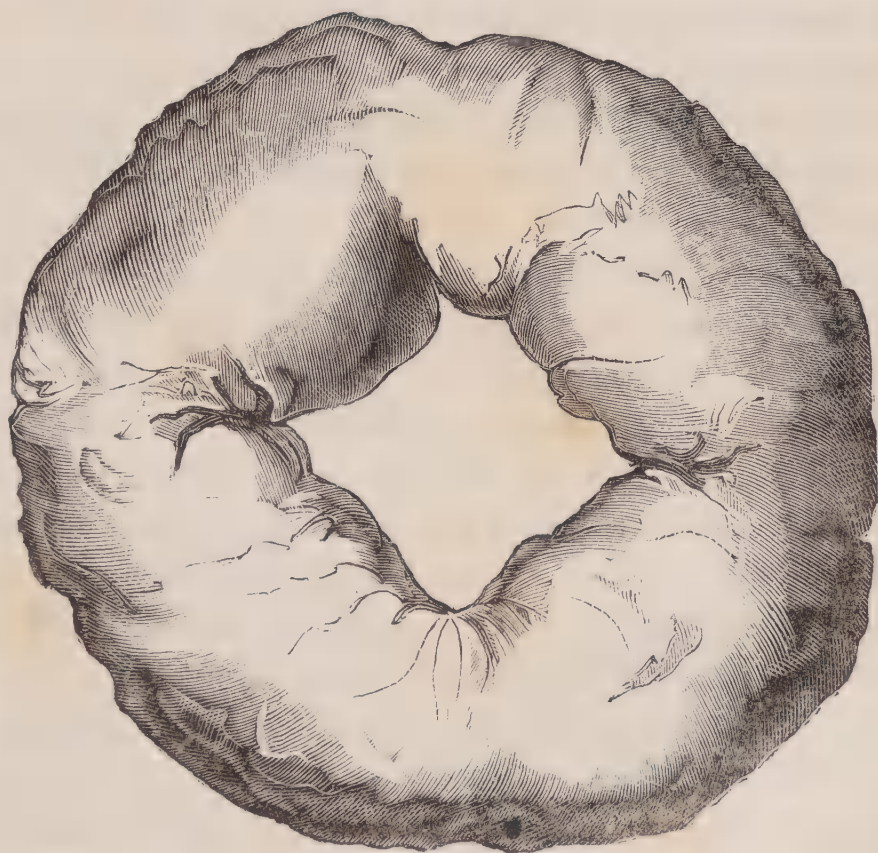
uterine contractions that follow its full doses: I gave five grains of the ergot, and in about ten minutes afterwards evidently perceived the uterus slightly ergotised. Considering the obstinacy of the os, I contented myself with carefully watching the continuation of the action of the ergot upon the uterus, and when it began to abate, repeated it in the same dose; this interval was in or about half an hour. In this way *three* doses of the ergot were given, and although I had the uterus so ergotised, that under ordinary circumstances, the os must have given way, (dilated), it still continued to resist the contractions of that viscus. I should have observed, that by this time, in consequence of the very protracted state of the labour, the patient had been much exhausted, so that interference was evidently called for. It may be said that bleeding, tartar emetic, and other relaxants should have been tried; I can only say I gave them the fullest consideration, and determined on the ergot in the way administered in preference; and I would here submit, that there are peculiarities attending sometimes particular cases, so devious from what are usually to be observed, that the treatment must be modified accordingly, in proof of which I believe I could not refer to any one more competent to give judgment on than yourself, from the very extensive practice afforded at the Coombe Lying-in Hospital. However, to resume, the os did not yield, but the head was propelled fully into the pelvis, pushing the cervix before it. In the course of the evening of the second day the patient's condition grew worse: she became delirious, the pulse quick and irregular; and in a word, she must have quickly sunk if interference had not been resorted to. To apply the forceps, I can only say, the circumstances of the case were such as decidedly to preclude it; there was nothing therefore but the crotchet, and with the hope, though possibly a faint one, of being able so to break up and dismember the child, as to transmit it through the os, the perforator was resorted to, and the cranium evacuated of its contents; the crotchet was then introduced, and traction of a very gentle nature made on one of the

parietal bones, for the purpose, if possible, of detaching it, in order to my intention as above stated. During this, however, a strong contraction of the uterus succeeded, when the head was at once expelled, carrying before it the os and a part of the cervix of the womb, the diameter of which measured about three inches and a half, which preparation is in the museum of the Coombe. The placenta came away in the usual manner, there was, however, considerable hæmorrhage, and such difficulty in getting the womb to contract, that the cold affusion became necessary. Two hours after the delivery I became much alarmed for my patient; jactitation, restlessness, difficulty of breathing, &c. I gave her a full anodyne, and having procured tranquillity, and given all the necessary instructions to a competent person as to the state of the uterus, which was padded, and other parts to be attended to, left her for the night. On my visit the next morning—and I confess to you it was not without strong apprehensions that I should have found her either dead or dying—to my surprise, she was sitting up in the bed eating her breakfast, expressing how comfortable her condition was compared with that of the preceding day.

“The remaining part of this case is very short, she recovered without a single bad symptom, and in the usual time. I have had an opportunity several times since of seeing this young woman, and of making an examination of the parts; the *present artificial os* is in the usual place, in the upper and anterior part of the vagina, of a pursed up or puckered appearance or feel; she menstruates, but irregularly, and most profusely, accompanied with large clots of coagula; and the case being extraordinary, I was induced to make inquiries from her with respect to sexual desires, which she informs me are nearly, if not entirely gone. She has never since proved pregnant. Such are the particulars of this curious case, and if I learn any thing of interest connected with it hereafter, I will gladly communicate it to you.”

I am not aware of any case similar to this being on record,

and its history is as important as it is interesting.* The state of the os, the manner in which the head came down, and the ultimate giving way of the obstructing part, seems as if nature pointed out the method by which relief could be obtained under similar circumstances. Knowing the scientific and sound practical knowledge of my colleague, and his expertness as an operator, I am convinced that he weighed well every circumstance attendant on the case, and acted accordingly. The ordinary relaxants, as he mentioned, were inadmissible, and artificial dilatation he found to be impracticable; but I believe, and indeed I have his word for it, that if such a case were to occur again in his practice, he would not give so long a deference to the powers of nature, but in proper time incise the os, a plan which I am sure would, under such circumstances, be attended with as favourable a result, and much less suffering to the mother, and perhaps with safety to the child.



* Since the above was written, two instances of separation of the os, as having occurred at the Rotundo Lying-in Hospital, are given in the Report of the Proceedings of the Pathological Society, in the last Number of the Dublin Journal.

This preparation exhibits the detached os ; its separated edge appears uniform, and rather even, presenting little or no trace of laceration, but the natural orifice seems fissured at two or three points ; there are no blood-vessels observable, but it is rather firm to the feel, and presents no appearance of œdema.

The method proposed for the performance of this operation is simple and easily accomplished. The patient should be placed on her left side, close to the edge of the bed, as in the ordinary obstetric position, the fore-finger of the left hand should be then carried to that part of the os or cervix intended to be cut, and a probe-pointed knife or bistoury conveyed cautiously along the finger in the vagina to the point mentioned, at which the os or cervix may be divided. This is done by gently insinuating the point of the instrument within the os, and pressing its cutting edge against the rim on each side, in the direction in which it is intended to be incised, the parts will give way readily before it ; and then cautiously giving the blade a withdrawing motion, the openings may be enlarged as much as may be deemed advisable. The bladder should be previously emptied, and if the incision be brought forwards great care should be taken to avoid its neck. The liquor amnii will escape after the first incision, and if the uterus act, the case may then be left to nature. Some tearing may take place during the passage of the head, but it is generally slight and confined within the limits of the vagina. (Ashwell.) With a view of better avoiding laceration, a crucial incision of the parts has been advised. Before attempting this operation a very careful examination should be made of the os ; for I recollect, on two occasions in this hospital, feeling a strong pulsation in the os, as if a large vessel coursed along a part of its rim. Indeed on the first occasion I imagined it was the funis that caused this pulsation, and it was not until after a careful examination, that I was satisfied as to its true source. There is seldom much blood lost. In Dr. Ashwell's and Mr. Tweedie's cases only a few drachms escaped, and the hæmorrhage which followed in Mr. Hugh

Carmichael's case cannot be said to have been altogether derived from the torn parts. If however the section of the parts be succeeded by a profuse hæmorrhage ; "or in the event of its being so long delayed that it might be highly improbable that the uterus would be competent to resume its office, or finally to effect its own delivery, then it should become a matter of deliberation with the practitioner, whether he should further assist by having recourse to the use of the forceps, or to the manual operation of delivering by the feet. As a general principle, it cannot be denied that the hand, being of softer texture, and itself endowed with feeling, would be the more gentle instrument ; on the contrary, if we suppose the foetal head considerably advanced, or deeply engaged in the cavity of the pelvis, and a sufficient extent of communication to have been made between the uterus and the vagina, it is evident that the application of the forceps might prove a much preferable measure."* If there should be fainting and collapse after the incision, stimulants, such as brandy and ammonia, may be freely given.

Hitherto I have considered this operation as applicable to certain cases of occlusion or rigidity of the os ; but there are others in which, under particular circumstances, I think it might be made available. It sometimes happens that the placenta is separated from the uterine parietes, but is confined within its cavity, in consequence of the os being *firmly and rigidly contracted*. This state is what is termed *incarcerated* placenta, and differs from the ordinary forms of irregular contraction of the uterus, in the contraction being more limited to the os and cervix. Many causes may produce this unfavorable state, but which fortunately is not very frequent. I recollect but two cases occurring since my appointment to this hospital: they were *extern* patients, and had been attended by *midwives*, and both were fatal. My friend, Mr. Armstrong, told me of another, that he had been called to lately in this city. The

* Davis's Operative Midwifery, page 98.

result of these cases is most generally fatal, the patient dying from the effects of a typhoid fever, probably excited by the putrescent mass retained in the system. When a portion of the placenta only is retained, a purulent discharge consequent upon inflammation is sometimes secreted, by the lining membrane of the uterus, and by which the particles of the intruding body are carried off. It is also recorded that the placenta has been altogether removed by absorption ; such cases are however extremely rare, and are essentially different from those now under consideration, in which the general termination is death. The first of these complications that came under my notice, was in a poor woman living in Engine-alley, and occurred shortly after I commenced my attendance here ; it was produced by the improper conduct of a midwife who broke the *funis*, in her efforts to extract the placenta, and probably thus irritated the os. The cord was ruptured close to its placental attachment, the os was not completely *closed* but *grasped tightly a small portion* of the placenta, which filled it like a plug ; it resisted all the means that were adopted for its relaxation. I was favoured in the treatment of the case by the advice and assistance of my friends Drs. Breen, Halahan, and Carmichael, men whose high professional characters are a sufficient guarantee, that all the ordinary and approved methods of treatment had been resorted to, and they tried them, but to no purpose, the patient died in a few days. The second instance was nearly similar. In both of these cases the women had arrived at the *full period* of gestation, to which state these remarks only apply.

In reflecting afterwards upon the case of the poor woman in Engine-alley, and which was particularly unfortunate, as she left a large young family of helpless orphans, the thought often flashed across my mind, that a simple incision of the os, would have enabled us to have taken a sufficient hold of the placenta to withdraw it. The os was fully within reach, and I would say, that, at least in this instance, the operation was practicable. I am well aware there would be greater difficulties attending it

here, than in the instance already alluded to; there is no doubt a greater depth of parts to be encountered, but it will be recollected I am speaking of cases where death is, I may say, inevitable; when the organ has receded, and its contractions have extended beyond the lower portions, then of course this operation would be out of the question, but I beg to be understood as referring only to instances in which the rigid contraction is limited to the *os*, and perhaps a portion of the cervix, a state which is readily discernible, and two examples of which, as before mentioned, came under my own observation. In such, *after all the ordinary means have been tried in vain*, and death seems certain, as a last resource, before the powers of life were too far exhausted, would this operation be admissible? Are there any circumstances that would render the after consequences of it, in this instance, much more dangerous than in the others? In *premature labours*, where the after-birth has been long retained, or where a portion of that body has been left in the uterus, and alarming febrile symptoms supervene; upon its being cast off, or removed, the fever abates and the patient recovers. Now in the case I have alluded to, the placenta, thrown off from the parietes of the womb, but *confined* in it by the contraction of the *os*, becomes putrescent, thus acts as a foreign body and lights up a typhus fever, which, unless the exciting cause be removed, will destroy the patient; here, when all the approved agents fail in procuring its removal, or abating the distress, ere it be too late, would it be justifiable to simply incise the *os*, so as to enable the operator to get at and remove the immediate cause of the patient's suffering and danger? I merely throw out the suggestion, without pledging myself to it, in the hope that practical men will calmly consider it, and if in an extreme case it should ever be the means of saving a mother's life, my only object will be fully attained.

ART. V.—*Remarks on the Epizootic Epidemic, or Disease prevalent amongst Cattle in the beginning of the present Year.*
Syn. Aphthous Disorder, cocote des nourrisseurs Fonzetto.
Ital. Fr. hitzigen Klauenseuche, Maulseuche, Maulweh.
Germ. By M. RAYER, Physician to La Charité. Translated
 by GEORGE WATSON, M. D., M. R. C. S., &c. &c.

THIS epizootic disorder prevails (Jan. 1839) not only in many dairies of Paris and at Alfort, but also in many of the surrounding communes. M. Le Blanc has observed it at La Chapelle Saint-Denis, and at Roquancourt near Versailles: others have seen it at Villejuif and near to Corbeil, Ruel, Nanterre, Saint-Ouen, Rambouillet, Mantes, &c. M. Fabre has observed it in the Canton of Geneva. After having studied this disease in the Vaccary of M. Poinsoy, I have been enabled, owing to the politeness of M. Renault, director of the School at Alfort, to observe it in his establishment, in cows, bulls, sows and pigs, goats and sheep. M. Le Blanc induced me to repeat these observations at La Chapelle Saint-Denis.

Up to the present moment the horses and dogs do not appear to have been attacked by the malady. In all the animals affected by it, and in particular in the cow, the disorder presents three stages; the primary fever, the eruption, and the desiccation or desquamation. The *primary fever* lasts two or three days. During this period, the cow is dull, the beatings of the heart are accelerated, the secretion of milk is diminished, and the milk itself, according to my observations and the researches of M. Guibourt, is slightly modified in the proportion of its elements. At a more advanced period it is *sometimes* mixed with a certain quantity of mucus or of pus. The hair is less smooth, and the base of the horns or ears is hot. The diseased cow sometimes has a cough, but the respiration is not sensibly quickened; the animal eats little or nothing; the thirst is increased; the fæcal matters, ordinarily natural, are occasionally liquid; sometimes the state of the animal,

on the whole, does not appear serious. The *eruption* declares itself the third or fourth day of invasion, on the *feet*, in the *mouth*, and on the *mammæ*. In the milch cows it manifests itself on the udder or dugs, on the internal border of the lips, upon the tongue, upon the gum covering the border of the jaw, and in the bifurcation of the hoofs. Upon the *dug*, the number of elevations varies from five or six to forty or more. The eruption generally *distinct*, is rarely *confluent*. These elevations, at first of the size of the head of a large pin, enlarged and stretched themselves out along the nipples or teats under the form of flattened pustules, sometimes marked with a point more depressed than the rest in their centre. The larger ones are about the dimension of a five sous piece (corresponding with our silver 4*d.*); but for the most part they have not quite so large a surface. These pustules, ordinarily isolated, are sometimes contiguous by some one point of their circumference, in such a manner as to resemble the figure of 8, or are united together in large irregular patches, hard and solid to the touch; they surpass but little the level of the skin, and their surface is of a white lightish yellow. They are sometimes encircled at their base by a very small rose coloured border, but they have not an areola, even towards the ninth day of the sickness, which corresponds ordinarily to the sixth and seventh day of the eruption, the period at which the greater number of the elevations have acquired their greatest dimensions.

Upon the body of the teats, the colour of these elevations is sometimes very nearly the same as that of the surrounding skin from which they are only distinguished by the slight circular prominence which they form; but more frequently they have a whiter tinge than the healthy skin; a tinge which appears to result from the imbibition by the epidermis of a serous humour, and sometimes by the deposit of a small pseudo-membranous layer. The serosity is never deposited in these elevations on the teats in such a way as to raise the epidermis and form small blisters or bullæ, as in the *vesicular* diseases properly so called,

or *bullous* affections. In no period of these elevations, and in particular in that which precedes their decline, do we find beneath the epidermis any circumscribed deposit of pus, so that if we have thought proper to make use, along with some authors, of the word *pustule* whereby to designate these elevations, it is necessary to premise that this expression is not taken in the rigorous etymological sense, but in a sense analogous to that which has caused it to be adopted, in order to designate the elevations of vaccinia and variola, which are not purulent in their *condition*, and in which a layer of false membrane, impregnated with a serous humour, is deposited beneath the epidermis; whatever it may be, in this epizootic eruptive fever, the sub-epidermic false membranous layer of the elevations is much less apparent than in variola, and the serous humour much less abundant. Sometimes even a needle or the point of the lancet, introduced between the epidermis and the skin, is scarcely moistened; but most frequently it is covered with a transparent, serous, yellowish liquid, which may also be collected in capillary tubes. When these elevations develop themselves on the extremity of the teat, they are almost always of a milky *dead white* (*blanc mat.*) colour, as if the epidermis was raised by a purulent humor; but this appearance is owing to a more complete imbibition by the epidermis, of a greater quantity of serosity, or of milk, when spirted from the end of the teat. In none of their stages, even in the highest degree of development, are these elevations accompanied by an engagement of the sub-cutaneous cellular tissue, as is the case with the true "*cow-pox*" pustule, according to Jenner.

The period of *general desiccation* of the eruption on the mammary glands commences towards the tenth or the twelfth day of the disease; sooner for some pustules, later for others. These elevations, when dried, have different aspects. When the epidermis, thickened by the imbibition of serous humour, dries up naturally, without being torn or scratched off, it wrinkles, acquires a yellow tinge, then a brown, and forms a lamellar

crust, which detaches itself towards the sixteenth or eighteenth day of the disorder, leaving bare a small smooth surface, of a greyish rose hue, *without cicatrix*. If the pustules have been disturbed by the effects of tractions made upon the dug, in milking the cow, or by other causes of rubbing, drops of blood and of serous humour flow out, and shortly a small brownish, irregular crust is formed by the epidermis, thickened by the dried blood and serous humour. The falling off of this species of crust is usually slower than that of the preceding ones. Lastly, it happens sometimes that the epidermis, moistened by serosity, is almost wholly detached by rubbing, from the surface of the elevation; there then results a *superficial excoriation* of a brownish-red, which is covered afterwards with a crust, formed in great measure by dried blood. But these superficial ulcerations do not degenerate into *ulcers*, similar to that which happens frequently as a sequel to the *true "cow-pox,"* according to Jenner.

The eruption of these pustules upon the dug is for the most part successive. However, there is in general only a very short interval between the epoch when each of them appears. Sometimes we find at once, upon the udder, pustules in their *prime*, and others in *desiccation*. But towards the sixteenth or eighteenth day all the pustules are ordinarily dried up, and in some the fall of the crusts has already taken place.

Although the eruption locates itself almost exclusively upon the teats, we observe sometimes pustules upon the body of the mamma. When they are numerous or agglomerated upon the teat, the cow experiences a very acute pain during the operation of milking; this pain diminishes in proportion as the pustules approach the period of desiccation.

The eruption is not limited to the udder of the cow: nearly at the same time when the pustules form upon the teats, and often before they show themselves there, there appears in the mouth, on the internal surface of the lips, upon the tongue, and oftener still upon the alveolar border of the upper and

lower jaw, a certain number of elevations, which, when they are distinct and isolated, appear like little flattened, circumscribed, ovoidal risings of the epithelium, having nearly the dimensions of the pustules of the teats, and which, when they are *confluent*, form *irregular patches*. These buccal elevations are evidently formed (as one can assure one's self by incising them with a bistoury) by the epithelium, thickened and impregnated by a serous humour, which has not entirely detached it from the external surface of the corium. The base of these elevations, or of these gingival or lingual patches, is sometimes surrounded by a small pale rose border; later, the altered epithelium is torn up, either by coming into contact with the teeth, or by friction with the food. Then we observe upon these parts *superficial exco-riations*; the corium is laid bare, but it is not ulcerated, whence has been derived the denomination of *aphthous disorder of the bovine tribe*, given by many veterinary surgeons to this ailment, although it attacks equally sheep, pigs, and goats.

When the *buccal eruption* declares itself, the mouth is hot, and the cow *slavers*; the flow is sometimes very remarkable, even when the eruption is limited to a small number of elevations, and without the mucous membrane of the mouth appearing red and sensibly inflamed. If the *buccal* eruption is considerable, at its decline, we observe a large desquamation of the point of the tongue and of the gums, at the same time a new epithelium forms. Occasionally the inflammation extends to the pharynx and to some parts of the intestinal canal. Lastly, an analogous eruption to that which we see upon the dugs and in the inside of the mouth, exhibits itself upon the portion of skin devoid of hair situated at the bottom of the interval which separates the *hoofs*. From the outset of the disorder, this part is hot and painful; when the cow is lying down, and wishes to get up, she can only do so with difficulty; her limbs totter, and appear stiff, and she brings the anterior and posterior limbs together. Afterwards the epidermis of the feet softens and cracks, either owing to the separation of the hoofs,

or by the contact with the dung or other litter; occasionally even the skin around the hoof, and that which is situated beneath it, is inflamed; and in analogous epizootics, the veterinary surgeons have witnessed the decadence of the hoof. Thus in the cow, the eruption shows itself upon the teats, in the inter-ungual furrow of the feet and in the interior of the mouth, that is to say, upon almost all the parts divested of hair, except the margin of the anus.

As to the *febrile phenomena* and the other general symptoms, they diminish usually when the eruption is out, and they cease towards its decline. During the period of desiccation or desquamation, the appetite is renewed, salivation is diminished, the evacuations become natural, and the cough, when it has been present, disappears, and the secretion of milk is re-established. The duration of the affection is about three weeks, and it almost always terminates favourably. All the cows grow thin; in all the secretion of milk, remarkably diminished during the course of the disorder, re-establishes itself slowly; and in many, the teats of which had been much inflamed, the secretion of milk has nearly entirely ceased.

Among the heifers, the mammary eruption is less pronounced than in the milch kine. *Amongst the bulls, the eruption in the mouth and in the hoof-furrow is the same as in the cows; the eruption is rare upon the scrotum of the bull.

In the actual epizootic of Paris, the benign character of the disease is such, that it appears the cure may be abandoned in the majority of cases to the efforts of nature. Although this affection progresses very regularly in the cow and the bull, we see occasionally some anomalies, such as successive eruptions, at intervals very distinct, swelling of the limbs, upon some parts *bullæ*, containing a reddish serosity; at other times also, the fall of the hair. This latter phenomenon has been the case with a cow belonging to M. Poincot; the hairs of the neck have fallen off to a very considerable extent.

This eruptive fever attacks equally the *pigs*. I saw at Alfort a sow which was suckling many young pigs, and in which the eruption was much greater than in any cow of the establishment. The teats and almost the whole surface of the mamma had been covered with the pustules. The skin, irregularly deprived of epidermis, was of a deep red, and studded with small patches of epidermis. The groin was entirely occupied by the eruption. All the young ones of this sow had both the groin and the feet covered with the same eruption. In general, it was much more abundant upon the feet of the pigs than upon those of cows or bulls; a circumstance also observed by M. Leblanc at La Chapelle Saint Denis.

Amongst the *sheep*, the fever which precedes the eruption has the greatest analogy with that observed in cows. The gingival eruption is also pretty much the same; but the eruption is very rare upon the mammæ of *sheep*. At Alfort, M. Renault has remarked that these animals only presented the eruption after it had been manifested upon the cows.

In *goats* the disorder appears very much the same as amongst the sheep.

The *horses and dogs*, in the establishment at Alfort, were not touched by the malady up to last Sunday, (Dec. 23, 1838,) and M. Leblanc has not observed this eruptive fever among these animals in his establishment, nor in his practice.

2. I conclude this notice of the prevalent epizootic by some remarks upon the modifications which it determines in the composition of the milk of the milch cows. When the eruption upon the udder is not great, and when there are no pustules upon the free extremity of the teats, and that there is no milky engorgement of the mammæ, the milk, as seen through the microscope, differs but little from ordinary milk; it presents not only isolated globules, like healthy milk, but also small irregular groups, formed of the same globules agglomerated, and a species of amorphous clouds, in which these globules seem entangled.

In cases where the eruption was considerable upon the teats, and especially at their extremity, or where the disorder had occasioned a mammary engorgement, (*cru des nourrisseurs*,) the milk contained, besides milky globules, a great number of mucous or purulent globules, which, by repose, were precipitated to the bottom of the vessel, whilst the milk globules mount to the surface. The proportion of mucous or of purulent globules in similar cases is very variable. It was so considerable in one of them, that in the microscopic field the milk globules appeared very rare. This milk, which M. Leblanc brought to me, and which was also examined by him, came from a sick cow at La Chapelle Saint Denis, the udder of which was covered with an eruption. They were only able to obtain a small quantity of milk from the teats, and that with difficulty: it appeared curdled to the naked eye. Some hours after having been collected it exhaled a most repulsive odour. M. Guibourt, to whom this milk was despatched for a further examination, considered it too much altered to allow of an analysis being undertaken with any prospect of a satisfactory result.

I here transcribe the comparative analysis which my learned colleague, M. Guibourt, has had the kindness to institute between the milk of a healthy cow and of one attacked by the aphthous disorder; the eruption on the dugs was slight:—

“ These two milks being left to themselves, that of the diseased cow has at two different periods separated more readily than the other, and the layer of cream appeared thicker and more yellow. In order to analyse these two milks, I heated them in a sand-bath in the same vessels in which they were held; I added to each twenty drops of acetic acid, the quantity requisite to develop the coagulation. The fluid was filtered, and the coagulum well washed and dried, previous to being treated with ether, which dissolved out the fatty matter; the evaporated serum was strongly troubled, and suffered a notable quantity of caseum to precipitate, which was separated from it

by cold water, and reunited to the caseum already obtained, and exhausted by the ether. Lastly, the fresh solution of the principles of the serum has been evaporated to dryness. Below are the results of this analytical experiment :

“ *Milk of Cow suffering with the Aphthous Malady,*
104 grs. 40.

“ Dry serum (sec.) . . .	6,05	5,795
Dry caseum (sec.) . . .	3,93	3,764
Butter	3,88	3,716
Water	90,54	86,725
	<hr/>	<hr/>
	104,40	100,000

“ *Milk of Cow which was healthy,* 99 grs. 15.

“ Dry serum	3,15	5,194
Dry caseum	3,82	3,853
Butter	3,42	3,459
Water	86,75	87,404
	<hr/>	<hr/>
	99,15	100,000

“ The two serums dried were equally sugared, and of an agreeable taste. The caseums were perfectly white and insipid. The butter from the affected cow was manifestly more coloured than the other, which at first induced me to suppose that there was much more of it.

“ *En résumé*, the milk of the affected cow gave more dry serum and butter, less water, and a little less caseum than the normal milk.”

3. A great number of analagous epizootics to the one we are at present contemplating, have been noted at different periods. I shall touch upon the principal ones.

A celebrated physician, Sagar,^a has given an excellent de-

^a Sagar (J. B. M.) *Libellus de Aphthis Pecorinis*, Anni, 1764, Viennæ, 1765, in 8vo.

scription of an aphthous epidemic which prevailed, in 1764, in Moravia, among the cows, goats, sheep, and pigs. This epizootic has the most intimate resemblance to the present one; the same fever of invasion, the same eruption in the inside of the mouth, extending sometimes to the pituitary membrane, occasionally with dyspnœa. Towards the seventh day, sometimes the ninth, the eleventh, the fourteenth, and even later, the period at which desquamation of the epithelium of the mouth was observed, the animals began to limp; tumours of greater or less magnitude formed between the hoofs, especially towards the back part of the foot; the tumours very soon suppurated, and if they were not opened, the pus made its way spontaneously. The ulcer, kept open by walking, and sometimes by larvæ of insects, was often slow to heal.

The milk of cows thus affected was richer in solid matter and thicker, it had entirely lost its sweet taste, and when it was placed at the fire it coagulated. According to Sagar, the use of the milk as food, developed, even among the animals which did not catch the disorder spontaneously, aphthous affections in the mouth and pharynx; and in this way man, dog, cat, and even fowls were sometimes visited by the malady. I have seen nothing similar in the prevailing epizootic.

Basaillon,^a a physician, has observed in the “generalité des Moulins,” and La fosse, fils,^b veterinary surgeon, in the environs of Paris, an aphthous epizootic, during the years 1775, 1776, and 1777. But it appears from their description, that accidents of a graver form than those we notice in the present epizootic, such as deep ulcerations in the mouth and on the tongue, were very frequent: “an aphthous epizootic, similar to that which is prevalent in Paris and its environs, visited the cows in the province

^a Instructions sur les Maladies épizootiques les plus familières a la généralité des Moulins, Moulins in 4to. 1787.

^b Dictionnaire raisonné d'Hippiatrique, Art. Aphthes.

d'Ivree in 1800. It has been described by Toggia,^a under the designation of *fonzetto*. Toggia remarks rightly, that this malady has been wrongly described under the title of *vajirole*, (*petite verole delavache*, small-pox of the cow.) "We know," says he, that the *vajirole* develops itself in the shape of small inflammatory tumours which rise above the surface of the skin, and especially on those parts divested of hair. In the *fonzetto*, these tumours were not observed on the mammæ, on the lips, nor in the nostrils of horned beasts; these parts are only attacked by *ulcers of an aphthous nature*, which are less frequent on the mammæ. We have even seen whole herds of cows perfectly exempt from aphthæ in this part."

In 1809, 1810, and 1811, an aphthous epizootic appeared in France, especially Normandy and Picardy, and was described by several veterinary surgeons. It was observed in the valley of Auge by Husard, Sen.^b in the environs of Paris by M. Girard, Sen.,^c in the department of the Rhone by the veterinaries of the School of Lyon,^d in the Ardennes by Dehain,^e in the East Pyrenees by Barrera,^f in the Haut-Rhin by Berhier, Jun.,^g in the department of Correze by Veilhau,^h in the Dept of Vosges by Mathieu,ⁱ who indepen-

^a Reflexions sur la Maladie appelée fonzetto, insérées dans Buniva: Memoire contenant les plus remarquables Notices Historiques and les Résultats les plus intéressants de ces Observations et Expériences relatives à l'Epizootic bovine hongroise, &c. in 12, p. 19.

^b Précis sur l'épizootic des Bœufs de la Vallée d'Auge, 1810.

^c Recueil de Médecine Vétérinaire, t. IV. p. 350, en note.

^d Comptes rendues de l'Ecole de Médecine d'Alfort, and de l'Ecole de Lyon, années 1811 et 1812.

^e Voyez Gohier, Mémoires sur la Chirurgie Vétérinaire, tom. II. p. 127.

^f Journal Général de Médecine de Sédillot, tom. XLIII. p. 196.

^g Mémoires publiés par la Société Royale d'Agriculture du Département de la Seine, tom. XVI. p. 116, 1313.

^h Mémoires de la Société Royale d'Agriculture, 1813, p. 48.

ⁱ Recueil de Médecine Vétérinaire, tom. XII. p. 65.

dently of the aphthous eruption of the mouth, describes the eruption on the feet and mammæ; it ravaged also Arriège, Tarn, Léman, and other parts of France, at that period.^a

The same epizootic was observed in Switzerland, by Saloz,^b in Italy by Leroy,^c and in Holland by Kraff. This aphthous epizootic appears to have had the greatest analogy with that which we see to day. *However the most part of the observers do not make mention of an eruption on the teats of the cows,*^d whether it be that it did not exist always, or whether they have neglected to notice it. I ought however to state that Berbier, Jun. had remarked that the teats of cows were sometimes affected; but he affirms that the epizootic only presents this character towards its close, or shortly before its disappearance. As to the lesions of the mouth and feet, they were the same as those we observe in the reigning epizootic—the same character of symptoms, the same benignity of the disorder.

Another aphthous epizootic, that MM. Peuchet, and Polette, have observed in 1819, in the Dept de l'Oise, presented a character of severity and of symptoms foreign to the actual disorder, such as violent inflammations of many parts of the head, sometimes an excessive swelling of the tongue, and of the sub-lingual glands, and of the nether jaw, which terminated frequently in suppuration: further, the affection of the feet, so general in the present epizootic, was not observed.

The same malady, after having prevailed through Switzerland in 1823, penetrated into Romagna in 1825. It was described by Lamberlicchi,^e who proposed to designate it under

^a Sainton. Correspondence de Fromaye de Fenyré, tom. IV. p. 263.—Leschevin, Annales d'Agriculture Francoise, tom. XLIX. p. 25.

^b Compte rendu d l'Ecole de Lyon, année 1812.

^c Mémoires publiés par la Société d'Agriculture, tom. XV. p. 60.

^d M. Rodet, Professeur de Médecine Vétérinaire, in his "Médecine der Vacut, in 8, Paris, 1829, Art. Aphthes, does not describe the eruption on the mammæ.

^e Lamberlicchi, Cenni theorico—pratici sull' esantema attalmenti ricomparso in Romagna, Pesaro, 1826.

the term, *stomato-interphalangeal exanthema* (*d'exanthème stomato-interphalangé.*) Towards this same epoch, this affection (known under the names of *klauen seuche*, *maul seuche*, or *maulweh*) was remarked in Germany.

In 1834, it appeared in Prussia, among the cows. About the same period, Casper^a observed in Bohemia *eruptions on the udders* of cows, and the *foot disease*. He does not allude to an aphthous eruption in the mouth. The inoculation of the matter of the pustules of the udder did not determine any eruption in an infant which was not vaccinated.

In 1835, the return of the *epizootic affection of the hoofs* (*Klanenseuche*) amongst the sheep, was in Prussia the occasion of a new publication of the ordonnance, which had been made in 1825, on the occurrence of an analogous epizootic. In the two months which followed the publishing of this ordonnance, there prevailed at Berlin, among the cows, not only the *epizootic hoof disease*, but also *eruptions on the udders* of young milch kine, which was called false cow-pox, (*Falsche Pocken.*)^b

4. Since the aphthous affection has prevailed in Paris among the cows, not a single case has proved the possibility of the *transmission of the disease to man*, by the use of the milk of the disordered cows.

Sagar relates that in the epizootic of 1764, almost all the monks of a convent were seized with the aphthous affection, after having made use of the milk of the diseased cows. The progress of the malady was the same in man, except that there was not developed on the feet an eruption analogous to that observed on the feet of the cows. “*Id præterea rarum fuit in aphthis, quod canes, cati gallinaccum genus ac homines ipsi*

^a Clarus and Radius, Beitrage sur praktischen Heilkunde. I Band. Extrait dans l'Algemeines Repertorium der Gesammten Deutschen Med. Chir. Journalistik. IX. Jahrgang, VII. Heft. Juli, 1835, p. 48.

^b Medicinische Zeitung, in 4, Berlin, 1835. See Constitutions Médicales des Mois Aout, Septembre, et Octobre.

ex usu lactis mulsi initio, aut in convalescentiâ ex uberibus vac-
carum hoc malo affectarum, morbum contrexerint, qui eundem
fere, quem in vaccis servabat decursum, præter quod tumoribus
ad talos, eorundemque effectibus casuerint hæc animalia.

“Hominibus pluribus, qui dicto modo aphthas hæditarunt
ipsis ego medicinam feci, et morbi decursum exactissimè
observavi.

“Quorum præcipua symptomata erant, deglutitio difficilis
tum solidorum, tum liquidorum, haud tamen impossibilis, calo-
rem præterea questi sunt, et ardorem faucium magnum qui
ad vesperam semper augebatur, his pro gargarismate, et potu
infusum specierum pectoralium melle rosato edulcoratum por-
rexì, and decidentibus aphthis residuam morbi materiam dato
electuario lenitivo cum rheo educendo ipsos perfectè curavi om-
nes, omneque alius ad talos præverti.

“Antequàm filum abrumpam, non possum non referre sin-
gularem casum huc maximè facientem.

“Conventus nimirum quidam virorum religiosorum totus
hoc morbo, ne unico fere excepto indubiè ex ursu laticiniorum,
afficiebatur, adquem initio mali accersitus morbi genus illico
cognoscens eum aliorum verti, seu præverti.”

Analogous observations have been made by *Berbier, Jun.*,^a
and by a veterinary of the vicinity of Lyons, in a similar epi-
zootic, which occurred in the Dept du Rhone, in 1811.

Toggia says, on the contrary, that in the epizootic *fonzetto*
(*maladie aptheuse*) which shewed itself in the province of *Ivrée*,
in 1800, the milk of the sick cows did not hurt any persons who
partook of it. *M. Mathieu*, in the aphthous epizootic of horned
beasts in the department of the Rhone, did not observe, either,
any bad effect from the use of the milk. For myself, since the
aphthous epizootic has sprung up in Paris, I have not observed

^a “Quelques personnes qui out fuit usage du lait de vaches malades, despores qui
en ont été nourris, ont été affectés de la Maladie.” (*Memoires publiés par la So-
ciété d'Agriculture du Departement de la Seine.*)

a single case of transmission of the malady by the use of the milk ; I have seen, it is true, a great number of sore throats, (angines tonsillaires,) with or devoid of stomatitis, and this at different periods of the year, in persons who used the milk chiefly at their breakfast ; but as the most part only made use of it after having boiled it or mixed it with tea or coffee, or with other alimentary substances, it appears to me difficult to suppose, that the milk could have any influence upon the development of these attacks. But we have fresh experiments which seem to confirm the opinion of Sagar. In 1834, when an aphthous epizootic occurred in Germany, MM. Wendenburgh, Tilgner, and Lehnhard, averred that they had seen several cases of transmission of this disorder to the human race, by the use of the milk. The subjoined fact appears still more conclusive: in the month of July of the current year, (1834,) Hertwig,^a the learned Professor of Veterinary Medicine at the University of Berlin, and two other veterinary doctors, (MM. Mann and Villain,) all three in perfect health, desirous of assuring themselves as to whether the use of the milk coming from the sick cows could develop the disease in man, performed upon themselves an experiment. "Each of us," relates M. Hertwig, "took, on the 26th of August, 1834, a pint of milk which had just been milked, and yet warm, from a cow affected with the aphthous complaint about five or six days, and which was very ill. We drank it slowly ; we repeated the same experiment on the 27th, the 28th, and the 30th of July. On the 28th I was seized with a slight fever, uneasiness in the limbs, little pain in the head ; my mouth was dry and hot, I experienced a sensation of itchininess on the skin of the two hands, and in the fingers. These symptoms continued till the 2nd of August, but they were so slight, that I paid but little attention to them : on that day all the mucous membrane of the mouth, and in particular, that of

^a Medicinische Zeitung, in 4to. Berlin, 1834, 26 Nov. s. 226.

the tongue, which was painful, swelled. On the 3rd of August there formed upon the tongue, chiefly upon its borders, and in smaller number upon the internal parietes of the cheeks and upon the lips, small vesicles, the size of the largest of which about equalled that of a split pea ; they were of a yellowish-white, and contained a whitish liquid, highly troubled, which flowed when they were pricked with a needle, and reproduced themselves afresh. At the same time there came upon the hands and fingers a crowd of vesicles ; the greater part had, at the moment of their appearance, the size of a millet grain ; they were firm to the touch, of a yellowish white, and occasioned a slight itchiness. On the 4th and 5th of August the vesicles of the mouth increased in size, and broke themselves the following days. Subsequently the epithelium detached itself completely from the affected places, leaving behind spots of a deep red, which disappeared little by little, from the sixth to the tenth day. The slight fever which had occurred the first days ceased after the appearance of the eruption ; but dating from this period up to the vanishing of the red spots, M. Hertwig experienced a constant burning pain through the mouth, could not masticate, or speak, or swallow the saliva, except with pain. The vesicles of the lips dried up, and covered themselves with thin, brownish crusts, which fell off on the tenth day from the appearance of the first vesicles. The progress of the vesicles which were developed on the hands was slower ; from the 4th to the 7th of August, they were filled with a liquid similar to troubled lymph ; they were larger and more confluent on some spots ; they broke, dried up, and the epidermis detached itself from the affected places about the 20th of August."

M. Mann, who had a slight fever the 30th of July, had also vesicles in the mouth, and especially upon the tongue, but fewer in number than M. Hertwig ; their progress was the same ; he had no eruption on the hands.

M. Villain, from the 1st to the 4th August, had a very intense fever, then an eruption of many vesicles in the mouth and

of a great quantity on the lips. In him also the steps of the disease were the same as in M. Hertwig: he had none on the hands.

5. I am not aware whether all the *inoculations of the humour of the vesicles* upon the human subject have been generally without result, as in the case of which Dr. Casper speaks. Four infants, inoculated by my colleague, M. Emery, have not experienced any effects, nor presented any eruption. One infant, inoculated by M. Bousquet and myself, has had fever on the third day, and an eruption of vesicles analogous to those of herpes, beneath the ear, on the internal surface of the lower lip, and upon the shoulder; these occurrences were soon dissipated. My colleague, M. Londe, has cited to me a case of breaking out upon the face, consequent on a like inoculation. Lastly, the following fact, which has been related to me by my colleague, M. Girar, deserves also to be noted. A man whose duty it was to throw injections into the mouth of a cow attacked by the aphthous disorder, seized its head; the animal threw some *saliva* upon his face and into the mouth of the man, which was half open, and very soon he was seized with an aphthous eruption on the tongue.

6. Since this epizootic arrived, and even during all the course of my practice, I have not observed in man any affection which I could regard as the *analogue* of the *aphthous disorder* of the bovine species. I will add, furthermore, that after having examined a very great number of accounts or histories of epidemics, I have only been able to detect a single instance which might be considered to have any very great analogy with the prevalent epizootic, although its symptoms were very severe, I refer to the *aphthous epidemic* which obtained in 1770 in the prison of Rouen, and which has been described by Lepecq de la Cloture.^a

^a Lepecq de la Cloture. Collections d'Observations sur les Maladies and Constitutions épidémiques, in 4to. Rouen, 1778, p. 386.

In conclusion, the eruption observed upon the cow of M. Poincot is the same affection as that which I have seen upon the cows at Alfort, and upon other cows at La Chapelle Saint Denis, and which exists at the present, not only in the environs of the capital, but in other districts of France. If in this disease the elevations on the udder, by their seat on the teats of the cows, by their flattened and circular form, have some resemblance to the picture which Sacco^a has given of "*cow-pox*," these elevations differ nevertheless from the pustules of the true *cow-pox* described by Jenner, in that they are not like these latter deep, umbilicated, surrounded by an inflammatory engorgement, nor *followed* with *ulcers* when they are irritated.

Epizootics of a like nature have been described under the name *aphthous malady*. They have spread among cows, pigs, sheep, &c. in Europe, and particularly in France, at different periods; but a great number of observers have not made any mention of the *eruption on the mammæ*.

Although the transmission of the malady to man by the use of the milk of the affected cows seems possible from the observations of Sagar and of Berbier, Jun., and from the experiments of Dertwig, it is, nevertheless, for the present an object of doubt. The solution of this problem is important to science, and may involve the health of the public.

ART. VI.—*Observations on Incontinence of Urine*. By CHARLES LENDRICK, M. D. T. C. D., Queen's Professor of the Practice of Medicine, Clinical Physician to Sir Patrick Dun's and Mercer's Hospitals, &c.

INCONTINENCE of urine is a disease that presents itself in various forms. Most practitioners of experience have met with numerous cases, where the habit of discharging the urine involuntarily

^a Sacco, Trattato di Vaccinazione, in 4to. Milana, 1800.

especially at night, has been acquired by the patient, and has proved an intolerable nuisance both to himself and family, as also difficult of management by the medical attendant. In many instances *enuresis* is doubtless only a part of a more extended disease of debility, but in general it is, at least at first, of an opposite nature and dependent on local *irritation*. Sir Charles Bell justly remarks, that in the majority of cases it depends on irritation of the inner surface of the bladder, just behind the prostate gland; and that by adopting means to cause the patient to sleep on the side or the belly, so as to avoid the supine posture and the contact of the irritating urine with the irritable surface, the nocturnal evacuation may be prevented. I have often succeeded in overcoming the disease, especially in children, by adopting this simple precaution. Incontinence of urine however generally depends on a more or less development of three agencies.

First in frequency and generally in date, may be mentioned an irritation of the parts about the neck of the bladder, to which the incontinence attendant on calculus, disease of the prostate gland, fungus, &c. may be attributed, but which may also exist independently of such causes as a primary affection. Sometimes the irritation is confined to the part of the bladder immediately behind the prostate gland, where the gland by its projection within the cavity of the viscus forms a pouch; and in these cases incontinence of urine is often the only symptom that attracts notice. In other instances the prostatic urethra partakes of the irritation, and the patient presents symptoms common to other diseases, among the rest irritability of the seminal vessels (after puberty) and frequent discharges of their contents. The involuntary evacuation of the urine and semen during sleep, seem indeed to be dependent on the same cause, and referrible to whatever produces irritation in the remote part of the urethra. Thus both are attendant on the morbid state of this portion of the passage, so well described by Mr. Abernethy.

The second cause, or rather aggravator of incontinence of

urine, depends on the *habit* which the parts acquire of discharging the contents of the bladder when the control of the will is removed, as during sleep; and in many cases of powerfully exciting the inclination during the day on the accumulation of the smallest portion of the usual stimulus; thus causing diseases designated by the terms “irritable bladder,” or “irritable urethra,” but which are in general confined to the part where the viscus unites with the canal. We often find, that in the day time, the patient is perfectly free from annoyance and able to retain his urine, a circumstance that proves the frequent independence of the disease of any paralytic affection. Its spasmodic nature is proved by the influence of an opiate at night in preventing the occurrence, and by the fact that where a ligature^a is applied so as to compress the penis, the patient often seems to those appointed to watch him to suffer great agony during sleep, while the portion of the passage beyond the ligature is found to be distended by the urine forcibly driven against the obstacle.

Thirdly—A more or less paralytic state of the neck of the bladder, which is sometimes the cause, is also occasionally the consequence of continued incontinence of urine. Thus incontinence and retention often alternate, especially in females.

These three causes, or aggravators of incontinence of urine—local irritation, the habit of involuntary evacuation, and debility, must be attentively considered, and the proportionate share of each in the disease duly estimated, before any attempt to treat it on scientific principles can be adopted. In general the above is the order in which these causes arise, and in protracted cases they are usually all developed. Thus Sir C. Bell’s plan of causing the patient to lie *off* the back during sleep, is well adapted to effect a cure in recent cases, and as accessory thereto in others. It ought therefore be had recourse to in

* This practice has been adopted by army surgeons in order to distinguish feigned from real enuresis.

every instance. Children can be easily subjected to the requisite restraint by a proper arrangement of the bed-apparatus; and adults are so anxious to get rid of the infirmity, that they willingly lend their aid by keeping themselves in the proper posture.

Where evidence is afforded of irritation and tenderness about the prostate gland, leeches may be applied there by means of Dr. Osborne's apparatus, or of a small and smooth gorget. Suppositories of opium, hemlock, and hyoscyamus may also be used. The urine must be kept as far as possible in a neutral state, not only by the use of acids or alkalies, according to the results of chemical testing, but also by avoiding whatever articles of food are found by experience to give the fluid a tendency either way. In no class of diseases is it more important than in those of the urinary organs to attend to idiosyncrasy, to arrange the habits accordingly, and to regulate the state of the bowels.

Bark, steel, cold bathing, &c., are frequently had recourse to in *enuresis*, and usually with but little effect; as indeed might be inferred from the rarity of its connexion with debility, except in advanced cases. Stimulants have been attended by more fortunate results, and have thus led to an erroneous view of the nature of the malady. The reason that *stimulants* succeed, seems in most cases to be similar to the principle on which Mr. Abernethy founded his practice—namely, that where morbid *irritation* exists, it may be subdued by stimulus or counter-irritation even applied to the diseased surface; provided care be taken to duly regulate the amount, to render it slight at first, and to increase the artificial as the morbid irritation subsides. In this way cubebs, buchu, &c. probably act. The best medicine of the kind is however the *uvæ ursi*. The patient may take half a pint of the decoction daily, and before each dose two pills formed of the resin of copaiba. Where the disease is of long continuance, and attended by difficulty of retaining the urine during the day, the twelfth of a grain of extract of

nux vomica may be added to each pill, and increased gradually to three times the quantity, if no unpleasant symptoms should be produced by its use. The principal efficacy of blisters to the loins and sacrum is probably attributable to the absorption of the cantharides, and the specific action on the urethra. Thus the tincture of lytta, taken internally, forms an excellent substitute for the copaiba pills, when the latter begin to lose their effect, especially in those cases where the seminal vessels partake of the irritation. The patient may commence with half a drachm daily, and increase by one-half, till slight ardor urinæ is produced, when the doses ought to be diminished.

Desault treated cases of this kind by means of a bougie, or even by retaining a flexible catheter in the bladder. The practice was nearly similar to that of Mr. Abernethy, although the principle was somewhat different. I have frequently observed much advantage to ensue from the urine being retained some time before going to bed, and being then carefully drawn off by the catheter. Not only the principles of Desault and Abernethy are thus acted on, but also a considerable effect is produced by the voluntary restraint of the evacuation, and by its subsequent accomplishment without any counter-exertion of the parts. Indeed in many cases of obstinate *enuresis*, the general health is so good, and the disease is so purely local, that local means form the only treatment we can employ with hopes of success, and of these, *injection of the bladder* unquestionably holds the first place.

The efficacy of injection of the bladder, in incontinence of urine, seems to depend on several circumstances—first, when water is injected of the temperature of the blood, it produces a sedative effect on the irritable surface, after the first impression of a foreign fluid has subsided—secondly, when at a subsequent stage the water is used cold, it produces probably a tonic as well as stimulating influence, and this at the proper period for stimulants—thirdly, the fluid dilutes, and thus removes the irritating portion of urine which sometimes stagnates in the pouch at the

neck of the bladder—fourthly, the bladder acquires during the operation of injecting the habit of resistance, by which it is enabled at other times to withstand the impulse to evacuation.

Injection of the bladder has fallen into disrepute with many practitioners, on account of the great principle that ought to influence us in stimulating irritable parts being neglected—namely, so to adopt the artificial irritant to the morbid irritability that the irritation produced shall be *moderate* in amount. It hence follows, that where there is much irritation already, our stimulus must be of the gentlest kind, and that our treatment can be active only when the disease verges on indolence. This treatment requires accurate examination of the constitutional peculiarities of the individual, of the progress of the disease, and of the effect produced by stimulants. In short our treatment must be *tentative*.

The patient ought to be somewhat accustomed to the introduction of instruments before injection of the bladder is attempted; and I have already mentioned the utility of such introduction as a part of the treatment, independently of it being preliminary to injection. The water ought to be injected through the catheter in its *flexible* state, whether introduced with or without the stylet. An inflexible instrument conveys every motion of the apparatus, or of the operator's hand to the urethra, and thus proves a source of irritation. The double barrelled silver catheter is indeed better adapted to washing out a dead bladder than a living one. A stop-cock should be attached to the flexible catheter after introduction, to which the injecting syringe is to be fitted so as to slide off and on, either for the purpose of allowing the fluid to escape, or for that of injecting a further quantity of water. A syringe capable of holding about five ounces is very manageable; but the small syringes, used as stomach pumps, produce too much irritation by their jerking movement, and a plain syringe of the size I have mentioned is the best. The piston ought to slide

smoothly, and be moveable by the finger and thumb. Of course the syringe ought to be perfectly clean, so as to avoid the risk of introducing a foreign body. An elastic gum bottle can scarcely be rendered sufficiently clean for the purpose.

It is not necessary that the water should be distilled. It ought, however, to be previously boiled, and poured carefully from the sediment. On the principle that has already been mentioned, the water ought to be blood warm at first, and, as irritation diminishes, its temperature should be lowered gradually to that of the atmosphere. The patient can seldom bear the injection of more than four or six ounces at first, and even this quantity must be thrown in slowly, the operator checking his hand the instant the patient complains of any uneasiness, or that the slightest impediment is felt. If the piston be pressed further under these circumstances, and before the resistance has had time to subside, the bladder is excited to spasmodic action, and the fluid must be allowed to escape. If the patient can retain the fluid without inconvenience, it may be allowed to remain, and be discharged at his leisure in the ordinary way—if it cause any uneasiness, a part, or even the whole, must be allowed to run off by the tube. No general rule can be laid down as to either the temperature or quantity of fluid or the time it should be retained, further than that the quantity ought never to exceed a pint, and that every thing should be regulated by the consideration of not causing pain to the patient.* If pain be produced the operation will do more harm than good. A similar rule applies as to the frequency of injection, which may vary from once per week to once daily. When the patient has become habituated to the

* An operator who adapts his hand with delicacy to the feelings of the patient, pressing gently when resistance has ceased; and suspending that pressure when an obstacle is felt, or perhaps allowing some of the water to escape, will, even during the first operation, be able in some cases to insinuate twelve or fourteen ounces into the bladder, while another is obliged to relinquish the attempt, after the rude and painful injection of two or three ounces, which the patient is forced to discharge at once.

operation, it is a very good plan to cause him to retain the urine in the evening as long as he can, then to draw it off and inject water; and finally to draw off the water by the catheter at bed-time. In this way the beneficial agency of the operation is perhaps brought to bear in the most effectual way.

Incontinence of urine in the female sex, depends frequently on causes not to be influenced by the practice here detailed; it may however prove valuable as a palliative. In one case of a very obstinate nature, some years ago, it proved highly beneficial; but the patient ceased to be under my care, before I could judge as to the probability of a cure being effected. Injection of the bladder is indeed to be understood from the preceding remarks, as only a part of the treatment of *enuresis* in either sex; and by no means to be had recourse to indiscriminately, or at once in every case, or to be persevered in when the aggravator of irritation. Like other remedies it will often act better if laid aside, and renewed on a future occasion. It is highly commended by Sir Charles Bell, as a palliative in incurable diseases of the bladder and calculus; and I fully concur with him, not only as to its value in such cases, but also as to its efficacy as a method of *cure*, (especially when aided by other remedies,) in those anomalous and yet tractable affections, which so often simulate these diseases. I make this assertion however with the qualification, that the aforesaid rules and limitations be borne in mind, both in the adoption or continuance of the practice, and in the mode of performing the operation.

ART. VII.—*Letter from* FLEETWOOD CHURCHILL, M.D.

TO THE EDITORS OF THE DUBLIN JOURNAL OF MEDICAL AND
CHEMICAL SCIENCE.

GENTLEMEN,

May I request the insertion of a few lines in the next Number of your Journal, for the purpose of rendering the history of “version” in these countries more complete, and of correcting an apparent error in one of the tables in my paper, to which you so kindly gave a place last month. I have commenced my quotations with the first English midwifery author, Thomas Raynald, but I have since obtained an extract from “The Breviarie of Health” &c. compiled by Andrew Boord otherwise called “Andreas Perforatus,” and remarkable as the founder of that class of the medical fraternity known as “Merry Andrews.” The colophon at the end of the first book is as follows, “Here endeth the first Booke, examined in Oxforde, in June, in the yeere of our Lord D.CCCC.XLVI. and in the raigne of our Soveraigne Lord King Henry the Eyght, King of England, Fraunce, and Ireland, the xxxviii. yeere.”

In the first book we find that “the 267 chapter doth shew of a woman labour or delivering.” The author says, “peradventure, the child is turned in the mother’s body, and that the head doth not come first, then there is great peril;” and he recommends, “If the head of the child does not come for the first, the midwife then must turne the childe that the head may come forth first, and let the midwife anoint hir hand with y^e oyle olive.”

The book is printed in black letter, and preceded Raynal’s work by eighty-eight years.

For this valuable addition to my paper, I am indebted to the learned research of my friend Dr. Aquilla Smith.

The error I wish to correct, is in the table shewing the mor-

talities of the operation. Mesdames Boivin and Lachapelle do not mention the number of mothers who died, so that in that column, instead of the 0 which stands opposite to their names, the words “not stated” should have been inserted. The table will then stand thus :

Authors.	Number of Version Cases.	Mothers lost.	Children lost.
Mad. La Chapelle, . . .	155	Not stated.	45
Mad. Boivin,	218	do.	48
Dr. Clarke,	48	6	35
Dr. Collins,	33	3	13
Dr. Cusack,	5	0	2
Mr. Gregory,	3	0	0
Dr. Beatty,	6	1	6
Dr. Churchill,	8	0	7
Professor Andrie, . . .	5	0	3
Dr. Klugé,	7	1	3
Dr. Küstner,	6	0	2
Dr. Adelman,	1	0	0
Dr. Boer,	26	0	10

By omitting the two upper numbers, we obtained the amount of cases in which the result to the mother is known.

I have the honour to be, Gentlemen,

Most faithfully yours,

F. CHURCHILL.

136, STEPHEN'S-GREEN, WEST.

ART. VIII. *Observations on Occlusion of the Vagina and Uterus, with its Means of Prevention, and the Operation necessary for its Removal.* By EVORY KENNEDY, M. D., Master of the Lying-in Hospital Dublin.

[Read before the Dublin Obstetrical Society.]

THE subject of occlusion of the vagina and uterus demands our attention ; first because a close investigation of its cause will shew us, that this very distressing affection may, in most

cases, by proper management, be prevented, and again, because from the difficulties and danger attending its cure, it is often either mismanaged, or totally neglected; the patient from a conviction that the case was beyond the assistance of art, being consigned to a life of misery and suffering.

It may be considered under the heads of partial and complete occlusion of the vagina; occlusion of the uterus; occlusion complicated with openings into the bladder and rectum.

It is not at present intended to treat of congenital occlusion, but of that form which is the result of inflammation occurring as consequent upon delivery or otherwise.

Partial occlusion of the vagina is to be met with, the product of ulcerative inflammation at certain parts of this canal, occupying more or less of its circumference, and which has terminated either by cicatrization, with an absolute deposit of new structure, constituting those bands which afford obstruction in labour, or by an adhesion of the opposite walls, and consequent cicatrization and contraction of the passage, throughout a greater or lesser extent of its course, causing a narrowing or diminution of calibre in the vaginal passage generally. The former is by no means an unfrequent occurrence, and one with which most practical men are conversant.

The principal inconveniences resulting from partial occlusion, are the interference with the vaginal functions, but more especially its obstructing the descent of the child in labour.

There is, in my museum, a preparation strongly illustrative of the necessity of dividing these bands when they offer any obstacle to the progress of the child's head; a plan of practice questioned by some of our midwifery authorities. In this case a falciform band spread round the posterior and lateral part of the vagina. The woman had been in violent labour, and the head resting upon the band for some time without advance: satisfied of its nature, and the necessity that existed for dividing it, I delayed operating for a moment, to afford my assistant an opportunity of examining. In this interval, the pains continuing

with great violence, the uterus became suddenly lacerated, and a triangular* flap of the os and neck of this body was thrown down into the vagina.

In a case in which two such bands had formed, seen with Dr. Labatt and Dr. J. Labatt, some years since, one band was divided, and whilst awaiting in expectation of the other yielding to the uterine efforts, the whole recto-vaginal septum gave way, and the rectum and vagina were thrown into one cavity, the rectal sphincter, however, remaining uninjured. The other form of partial occlusion, or that in which the canal becomes generally contracted, is more rare in its occurrence. In some the vagina is only slightly diminished in calibre and its functions consequently little interfered with, a subsequent labour inducing relaxation and development of the canal. In others, however, the passage is scarcely pervious to an ordinary sized catheter, and even the introduction of this is attended with pain and difficulty; in two cases of this kind which we met with, the external orifice was of its natural size, but about eight or ten lines within it the canal became suddenly narrowed, in both instances being pervious towards the recto-vaginal wall. In fact had it not been for the regular discharge of the menses, the contraction was so great in both, that they might have been mistaken for cases of complete occlusion.

I lately had charge, along with Dr. Johnson, of a patient in her first labour, who had a contraction of the vagina similar to the last described, but which was congenital; coition was attended with great suffering, and she had been treated by the introduction of sponges to admit of this: the first stage of labour lasted for forty-eight hours; at first the finger could with much difficulty be introduced, but the vagina gradually dilated to the fullest extent, when ergot of rye was administered, and the child expelled without further difficulty, by the uterine efforts.

In complete occlusion of the vagina, the adhesion may exist

* Dr. Kennedy exhibited this preparation to the Society.

between the opposite walls of the vagina, only at a certain part leaving an upper and lower chamber, separated by the adherent part.

A case of this kind I saw lately with Dr. O'Reilly, the effect of inflammation, after the removal of a uterine polypus, to which I shall afterwards allude, and a similar case, some time since with Mr. Mansfield of Camden-street, the effect of inflammation consequent upon difficult labour; but in the latter, although the os uteri was perfectly locked up, yet, as the symptoms were not sufficiently urgent to require an operation, nothing was done. The age of this individual may explain the absence of suffering, as she was rather advanced in life, and possibly, as the period approached at which the menses were about to cease spontaneously, their suppression might not be attended with the same extent of constitutional or local disturbance. Many cases of this kind have been sent up from different parts of the country, for treatment in our diseases of female ward, the result of difficult or instrumental delivery.

The walls may be adherent throughout their whole extent, obliterating entirely the vaginal canal; the bladder and the rectum being separated merely by the now common or coherent walling of the vagina; of course the latter is a case much more serious and difficult to be managed than the former.

In the great majority of cases, this disease might, with very little attention and management, be prevented; the midwifery practitioner should anticipate its occurrence in every case in which there has been tedious or difficult labour, laceration of the perineum, vagina, or uterus, or where instruments have been had recourse to, and symptoms of inflammation or sloughing of the vagina evince themselves after delivery, and even in cases of operation in the vagina, unconnected with labour, as polypus, &c. The result of the case of the latter kind already alluded to shews that it should be guarded against. The duty of attending to the state of the vagina after delivery, and under the circumstance alluded to, is but too frequently entrusted to the attending,

perhaps ignorant, nursetender, and hence arises the train of miseries alluded to. The practitioners in all such cases, after the immediate irritation has subsided, and the slough separated, should direct, in addition to frequent injections, the daily introduction into the vagina of some resisting cylindrical substance; a mould tallow or wax candle rounded at the point, and immersed for a moment in tepid water, answers the purpose very well; and when the parts are inclined to cicatrize and contract, this should be cut to fit the vagina, and left constantly within it, even this however, the practitioner should not entrust entirely to the nursetender, but occasionally explore the vagina to satisfy himself it is pervious, and that the os uteri can be distinctly felt. If bands or contractions form, he should divide them early with a bistoury, before they encroach much upon the canal, and retain the dilating plug constantly within the vagina; by this simple process he will prevent the occurrence of these adhesions.

When, however, either by neglect upon our own part or that of others, occlusion is allowed to occur, how is it to be remedied? and in the first place, what necessity exists for interference? In those cases in which the obstruction occurs merely at a point, the adherent portion of the walls may be separated without any great degree of difficulty, by careful division with a scalpel; more especially if the menstuous fluid have accumulated above the obstructed part, it serving as a wedge to separate the walls, indicate the part to cut towards; the fluid sometimes distending the uterus upwards to a very considerable size, at the same time that it protrudes the obstructed floor downwards, and this accumulation is usually accompanied with much suffering, principally periodic. The directions of practical authorities is to await the accumulation of this fluid, in any operation we undertake, as it facilitates the operation and renders it more safe. This is to a certain extent a useful hint, as far as it deters practitioners from rash and unnecessary interference, but it is *wrong* to allow this consideration to influence us by inducing us to *withhold relief from all patients, in whom no*

accumulation occurs, no matter to what extent they may suffer in consequence of obstructed vagina. It is a mistaken idea, but one in general acceptation, that the inconveniences resulting from obstruction in the vaginal passage, are productive of suffering to the patient *merely* by the *menstruous secretion accumulating* above the point of obstruction, and that hence arise the lumbar and pelvic pains, abdominal spasms, nausea, vomiting, &c. *Whereas the fact is, that this train of symptoms will occur at the return of the menstrual period, without the slightest accumulation or secretion taking place.* It is a curious law of nature, which I have observed in this, as in some other cases, that the functions of an organ should be suspended, as it were by an intelligence possessed by the organ of the futility of their performance; thus in some cases no secretion is formed by the uterus, because from the obstruction to its passage in the vagina, it cannot escape, and again remove the obstruction by rendering the vagina pervious, and the uterus immediately re-commences its functions. Now, whilst I would anxiously caution the junior practitioner against unnecessarily having recourse to so dangerous and serious an operation as that of rendering pervious the completely occluded vagina, yet I should wish him not to be led astray by the dictum of writers on this subject, in expecting to find an accumulation of the menstruous fluid beyond the point of obstruction, where the symptoms alluded to occur, or in withholding the necessary operation awaiting the proofs of this accumulation having occurred; else, he will often be disappointed, and his patient obliged to support, year after year, a life of misery and suffering, eventually sinking under it, or undergoing an operation, in addition to what she has already suffered, when ill able to bear it; and this, although the effect of a well-timed operation would have been to restore the functions of the uterus, relieve the patient from her excruciating torment, and re-establish her general health.

Let it not be supposed however, that this operation is not one of a very serious nature to the patient, from the risk in-

volved, as well as the pain attending it ; or that it is not a very difficult one to the operator. The thinness of the texture to be divided, the danger of wounding the bladder on one side, and the rectum upon the other, or getting into the peritoneal cavity, above, the depth of parts in which the operation is to be performed, and the confined space afforded to the operator, renders it as difficult and hazardous as any operation that can by possibility be undertaken. When the operation has been determined upon, I would advise the operator to be provided with several spatulæ of different breadth and thickness, either of wood or ivory, as well as with knives of different forms, at least two well rounded scalpels, with handles seven or eight inches long, a double edged scalpel, a blunt-point bistoury, muffled to within half an inch of its point, two or three brass tractors to separate the vulva, and a sponge, fastened to a stick about seven inches long. The patient is now to be placed in the usual position for lithotomy on the edge of a bed or table ; the bladder and rectum having been emptied, the forefinger of the left hand is to be introduced into the rectum, and a careful dissection made from side to side, in the line of the vagina, through the coherent walling ; the finger in the rectum indicating the correctness of the line in which we cut ; and if a doubt exist in our minds, as to the course we should pursue, always directing the knife more towards the rectum than the bladder, as a wound in it is productive of less inconvenience. Having cut in this manner as far in as is safe, carefully withdrawing the knife, and inserting one finger to ascertain the situation between each incision, when the incision becomes deeper, we introduce a narrow spatula and cut carefully upon it, with the knife parallel to the blade, widening it from side to side as we proceed. Much may also be done by separating the cellular tissue connecting the walls, by the finger or spatula as a dilator, gaining a central separation, and afterwards cutting it larger from side to side with the scalpel. Where the textures are too dense for this, then the double edged scalpel may be used for the same

purpose, and the sides enlarged with the ordinary scalpel or bistoury; we shall find the latter instrument particularly useful where bands of the cellular tissue, more resisting in their nature, remain laterally, and which ought to be carefully divided by it. By persisting in this manner, cautiously and carefully dissecting the structures, we at length get up to the uterus, the lips of which we now detect bounding the cavity above; if menstruous fluid be accumulated above, we are satisfied of the operation being completed by its escape, which in its dark and grumous character contrasts with the blood escaping from the vessels; where this is not the case, we must carefully pass a round or metallic catheter into the os uteri, if this be pervious, and if not we must either proceed to render it so, or await for some days the further dilatation of the vagina, before taking this step. I have been induced to be thus explicit in describing the method of operation, because I esteem our success in these cases to depend entirely upon the manner of operation; nor can I avoid alluding to an operation on occluded vagina, recently reported in the *Medical Gazette*, in which the plan ordinarily recommended, that of operating by forcing up the trochar at random, was adopted by one of the most eminent and distinguished operators in the sister kingdom. The trochar may with propriety be used for giving escape to menstruous accumulation, by perforating a hymen or slight vaginal band, but it appears to me to be quite unfitted in case of extensive adhesion, where the nicest and most careful dissection is necessary to separate the coherent membranous wall, with such risk of opening into the adjoining cavities.

I shall now briefly state in detail, some of the cases in which the above operation was had recourse to.

1st February. Ellen Hughes, æt. 27, was delivered eight months ago, of a dead child (her first) by instruments, probably the perforator, after a labour of thirty hours. She was confined to bed for a month after delivery, with pain of abdomen and vagina, after which time the vagina became closed,

and since that she has not menstruated. At every menstrual period, she has pains in her back, accompanied, she says, with the formation of two lumps in the lower part of abdomen, which usually remain for two or three days, and then gradually disappear. Has no headach or other symptom at these times. Bowels generally costive. It is now about three weeks since the last menstrual period. Some purgative medicine was administered, and on the 8th February, the operation was performed of opening the vagina, from a short distance from the vulva up to the os uteri, at least three inches and a-half; throughout the whole of which extent, adhesion existed. This was successfully done by cautious touches of the knife, whilst the finger of the left hand was for the most part in the rectum, to shew how far they might be directed towards the gut. The os uteri itself, which was also found occluded, was then pierced. Some difficulty was experienced in detecting its situation, and the incisions into the vagina were in consequence carried rather higher towards the cul de sac, than was necessary. After the operation she was ordered Cal. gr. ss. cret. gr. ii. 3tia qq. h. -

9th February. Slept well last night; pulse 120; no tenderness on pressure over the abdomen. Bowels have not acted since the operation. Passed water freely; on examination, a fluid covers the finger, similar to retained menstruous secretion. A catheter was passed into the os uteri: took nine powders.

Ordered to be syringed with tepid water twice a day. Oil draught; continue powders.

10th February. Says she is quite well; pulse 120; gums slightly touched; no tenderness or pain in abdomen; slept well during the night; respiration rather hurried; headaches. Syringing caused some smarting, and brought away copious discharge.

Continue powders.

11th February. Pulse 112; no tenderness in abdomen; passes water freely; bowels moved without pain.

Omit powders. Oil draught.

12th February. Constitutional symptoms have subsided.

From this period the treatment consisted in passing the finger at first, afterwards leaving a candle, (No. 6,) in the vagina, and great attention to cleanliness. The catheter was occasionally passed into the os uteri.

23rd. February. A tent was introduced into the os uteri, and on examination, a very minute ulcerated opening was found just within the sphincter of the vagina leading into the rectum. Tenderness of the vagina on pressing the candle has almost gone.

1st March. Complained last night of pain in back; some red discharge appears on the end of the candle, when withdrawn from the vagina; os uteri admits a catheter to a considerable depth.

In consequence of the attempt at menstruation, she was given a hip bath each night, and iron was administered, first in the form of the sulphate, and afterwards in Griffith's mixture. The slight red discharge continued three days and then disappeared; some irregularity of the upper part of the vagina continuing, in which, apparently, the passage into the uterus is obstructed. The caustic pencil was freely applied, the irregular surface destroyed, and the uterine aperture rendered free and pervious, by the frequent introduction of the catheter.

In this case the vagina shews but little tendency to contract again. The passage easily admits a mould six. Patient returned to the country quite well, but rather sooner than we could have wished, as these cases require watching for some time, to prevent the recontraction of the vagina.

Here then we have menstruation setting in within three weeks after the operation, and although the vagina had been occluded for eight months, yet no appearance of accumulation was per-

ceptible. The mercury was administered here, as shall be seen in others, as a precaution against peritoneal inflammation, an occurrence so much to be dreaded in these cases; and which, when it sets in, there is scarcely time to combat by producing mercurialization; therefore in these operations, as well as in operations in labour, when I have reason to anticipate the occurrence of peritonitis, I generally adopt the precaution of administering small and repeated doses of mercury, before the symptoms I dread shew themselves; a practice to which, particularly when a peritoneal tendency has been prevalent, and operations have been necessary, I feel convinced I owe the safety of many valuable lives. In some of these cases after operation there is an obstinate disposition to recontract, requiring our protracted and assiduous care to prevent this. The case of Bridget Fitzsimmons was one of this nature.

Bridget Fitzsimmons, æt. 28, married four years, delivered of her first child, a boy, by instruments, after a labour of five days, subsequently she suffered much from vaginal pain and tenderness, and was confined for two months; did not notice closing of vagina till about three months ago, when her husband directed her attention to it; never menstruated since, but has suffered from head symptoms; vagina completely closed, excepting a small canal anteriorly admitting a small quill.

On the 28th May, the vagina was opened, as above described, and the os uteri punctured. She was then put on the use of calomel till all danger of peritonitis had passed away. A candle was thenceforth constantly employed to dilate the vagina; and a very remarkable feature in the case was, the rapidity with which the canal closed when she allowed it to slip out, or when a fresh one was not introduced before the former had melted down. From these causes the vagina frequently diminished so much in size, in thirty-six or forty-eight hours, as instead of admitting a mould six, to require even a dipt to be paired, before it could be passed. When the vagina was so far dilated, as to receive

with ease, a mould four, she was permitted to go about July 27th, at the same time being cautioned to continue its use.

Nature occasionally makes an effort to relieve herself, in these cases of obstruction by the discharge of the menstruous secretion, by a curious process ; a case of this kind we had in a woman named Bowles, which, although it proved but temporary, was interesting, as proving the resources occasionally available in diseased states.

Anne Bowles, admitted January, 1837, with occluded vagina ; was delivered September, 1835, of her first child, followed by peritonitic symptoms. The vagina continued sore for some weeks ; had subsequently an attack of continued fever. States that about six months ago, on the 3rd of the month, she was seized with pains resembling labour pains, which continued for three days and nights, and then ceased ; (had always menstruated at the beginning of the month, but not at all since delivery.) These pains have continued to recur regularly every month ; vagina was completely obliterated ; there is a fungous growth about the meatus urinarius, which is very painful ; the uterus can be distinctly felt distended midway between pubis and umbilicus. In this state she continued (the fungus having been destroyed by caustic) until March, the paroxysms of pain becoming more frequent and severe, requiring opium pretty freely for relief. She now complained of an uneasy forcing sensation in the rectum, and on passing the finger into the gut, a tumour not unlike the lower part of the uterus could be felt ; this sensation became more distressing, and eventually, powerfully expulsive pains set in, during each of which the right labium was protruded in a conical form, the apex of which shortly assumed a bluish colour. She now suffered so much, and as on examination of the protruded tumour it was found to contain fluid, supposed to be menstruous, a bistoury was passed into it, and exit given to a large quantity of menstruous fluid, which continued to flow for two days, after which it became seemingly mixed with pus, which continued

discharging up to April 4th. She subsequently suffered from peritonitis ; was treated by leeches and mercury, and left the Hospital April 22nd, the communication existing between the vagina and tumour : readmitted September 30, the opening in the labium having become obliterated ; suffered again as before, until after a severe paroxysm, menstrual fluid escaped *per rectum*. November 25th, the vagina was opened up to the os uteri, by the operation already described, treated with dossils of lint, and eventually the introduction of candles gradually increasing the size.

The os uteri is occasionally occluded, the result of adhesive inflammation without the vagina participating in it. This is to be ascertained by speculum examination, and the impossibility of passing a catheter of any size into the os. In two cases of this kind which occurred to me, the menses were suppressed, and no accumulation took place, although several months had passed over. The opening in one case was made with a trochar, passed in the course of the uterine cavity ; in the other with a double-edged knife, and no inconvenience resulted in either. From the operation, the menses were speedily secreted and discharged, in one within a fortnight after the operation ; in both, however, a strong tendency existed to re-occlusion, which was only prevented by keeping a tent for some weeks in the opening and by introducing gradually increasing-sized catheters for some time afterwards. We have already seen the difficulty attending the keeping the os and vagina *open* after operation. Since writing the above, I detected occlusion of the uterus in its commencing stage, in the case of Curtis, one of the women from whom the os uteri separated and was thrown off before the head of the child in labour, and which I reported at a former meeting of this society ; in her the lymphic deposit was not completely organized, therefore the adhesions were easily broken through by the finger, and on doing this a considerable quantity of pus escaped, which had evidently been secreted and accumulating within the uterus, above the point of adhesion in its neck ; sub-

sequently a band of adhesion in the vagina near the os was divided, and she recovered perfectly.

After these operations, as well in the os uteri as vagina, although the divided surface will cicatrize healthily, an inclination is sometimes taken on to ulcerative inflammation at certain points, leaving communications with the adjoining cavities. We have seen, in the case of Hughes, that this took place to a small extent in the recto-vaginal septum, leaving an opening into the rectum. This however is a matter of little moment or inconvenience, and if treatment be necessary, can be easily remedied by diminishing the aperture by the application of the cautery or caustic.

It is of much more serious moment, however, if it extend to the vesico-vaginal septum, and attended with still greater danger, if it occur after puncturing the uterus ; as here, the ulcer may open into the pelvic cavity through the peritoneum. This occurred in the case which follows :

Fitzsimmons, whose case is above referred to, returned to hospital in September, having through neglect allowed the passage to become again contracted to very small dimensions ; the os was again quite imperforate ; candles of successive sizes were used to dilate the vagina, which succeeded without any operation ; and on the 28th the os was again pierced, and opened to about its natural size, and a dossil of lint left in the opening.

30th. Some menstrual secretion adheres to the catheter when introduced into the artificial os uteri.

October 1st. In passing up the catheter cautiously this morning, it went up so far as to leave no doubt that it must have got into the cavity of the peritoneum through an ulcerated opening ; she immediately complained of pain of abdomen and loins, similar to what she usually experienced during the menstrual period ; she was immediately placed under the most active mercurial treatment.

About 5 o'clock she was reported to be very ill ; pulse was then 112, and small and hard ; she had had rigor during the day ; skin was hot ; tongue foul ; excessive tenderness on pressure existed over the uterus, and in the right iliac region ; she was

bled in the upright posture to $\frac{3}{4}$ xx., when her pulse failed; twenty-seven leeches were then applied to the abdomen; at 9 o'clock a warm bath was given; after which mercurial inunction was employed every two hours. One grain of calomel and one grain Dover's powder given every hour.

11 P. M. Pulse down to 80, and is much fuller. Found relief from the bath, but still intense tenderness exists all over the abdomen.

Contr. remedia. Two and a-half dozen of leeches to abdomen. Hs. anodynus.

October 2nd. Tenderness a good deal relieved, but in the left iliac is still considerable. Mercury and depletion were pushed rapidly, and she was salivated on the morning of the 3rd, after which she gradually improved.

On the 11th. Vagina is now found to have contracted at one part as if encircled by a cord.

A small candle was passed, and gradually a larger one, until a mould four was easily introduced.

November 18th. Complains of pain in the loins and head, and symptoms of the approach of the catamenia. On withdrawing the candle, a small quantity of menstrual fluid is found on its end. The candle has been constantly used since last report, but still the same difficulty on keeping the vagina permanently dilated has been experienced, as formerly remarked. Even when fully dilated, so as to admit a mould two, if neglected for twenty-four hours, contracts so as to refuse admittance to the point of the second finger, and it requires seventy-two hours to enlarge it to the same size.

For the catamenial symptoms, she was ordered a hip bath each night, which increases the flow of the menses.

December 8th. Vagina admits easily a large wax candle, which seems to agree well with her, and does not prevent her going about.

Allowed to go out on condition that she use the candle constantly, and return occasionally.

Here then ulceration had evidently set in on the neck of the uterus, consequent on its perforation, and induced such a diseased state of the parts, as caused the catheter to pass into the cavity of the peritoneum, and that without the slightest force being used in its introduction, it having been passed in the same way, day after day, from the period of the second opening of the os, up to the sixth day afterwards, when this occurred.

The woman no doubt owed her recovery to the nature of the disease being so instantly discovered, and actively treated. This case is further illustrative of the necessity of extreme caution as well in our after treatment, in retaining the os uteri pervious by the introduction of catheters, as during the moment of the operation for opening the occluded os.

Fitzsimmons' case is further illustrative of the difficulty attending the keeping the newly formed vagina dilated.

The following case shews the necessity of attending to retaining the vagina pervious after operation unconnected with labour, and in diseased states of this canal, or of the uterus generally. It occurred after the operation for the extraction of a uterine polypus, in which I assisted my friend Doctor O'Reilly.

“ DEAR SIR,

“ The subject of the case you require was a widow, æt. 48, who at the commencement of the present year observed an exceedingly offensive and copious vaginal discharge.

“ On examination I found a polypus of the magnitude of an infantile head, impeding the functions of the rectum, and exciting irritation of the neck of the bladder. It presented itself between the labiæ externæ, and had a very thick stem attaching it to the inside of the body of the uterus, there was occasional hæmorrhage, and as she was declining much in health, from the irritation and discharge, a ligature was applied, by means of the double canula, which was tightened every day, until the fourth day, when the ligature gave way, and the removal was effected by

your curved uterine scissors ; she speedily recovered her health and strength. From a feeling of delicacy on her part, my attendance was dispensed with, and she committed herself wholly to the care of a nurse-tender. Three months subsequently I was sent for, when she complained of a bearing down pain, similar to what she felt on a former occasion, at the same time observing, that she attributed those pains to an interruption to the menstrual discharge; not having seen any appearance since the removal of polypus, this circumstance, together with my hearing the night previously a part of your paper on the subject of occlusion of vagina the result of various causes, distinct from congenital obstruction, led me to anticipate occlusion, and finding on examination that the vagina was occluded, I sought for your assistance, when the operation was performed, by cautiously cutting through, and separating about one inch and a-half of the cohering walls; exit was given to nearly one pint of retained menses, which possessed all the characters peculiar to that fluid. Although I have had to make an incision through an exceedingly dense imperforate hymen, still the danger or difficulty attending such will bear no comparison with the operation for occlusion, the result of adhesion of the vaginal walls, the puckered cicatrices and consolidated vagina bringing into contiguity the urethra and rectum, and every movement of your scalpel endangering both. The candle plug was subsequently introduced, and the parts kept pervious until cicatrization was completed.

“ Yours,

“ C. O'REILLY.”

The last form of occlusion of the vagina is that in which it is combined with lesion of the vaginal walls, leaving openings into the rectum or bladder; this is a very complicated and unfortunate case, generally allowing little to be done in the way of operation, at least with a view to cure, although it may, by management, be much relieved.

In the first case, where the communication exists with the

rectum, there is little comparative inconvenience. As has been explained by Dr. O'Beirne, the bowel is possessed of a power of retaining the fæces in many cases, although the sphincter ani or recto-vaginal septum be injured. This I have constantly observed, and although during the effort of emptying the lower bowels or otherwise, a portion of fæces may make its way into the vagina, little attention is paid to it. I have always noticed, however, in these cases, that when the patient took aperient medicine, more especially when the sphincter was injured, she lost the power of retaining the fæces, for the time the medicine was in operation, and in injury of the septum it made its way more freely into the vagina. The state of constitutional health will also influence these cases, in their power of retaining the fæces ; and tonic regimen, with the shower bath, sea bathing, or dashing cold water on the loins, will prove favourable to them ; operations, or mechanical contrivances, are therefore seldom necessary : pad and bandage sometimes afford relief, particularly if combined with uterine displacement.

The case in which fistula of the bladder is combined with occlusion of the vagina, is, however, very distressing, and generally attended with excoriation of the vulva and thighs, and all the miserable circumstances attendant on vesico-vaginal fistula, in addition to those of occlusion.

The irritation of the parts must be removed before any plan of treatment is adopted ; this is to be done by attending to the position in which the urine escapes least, which is generally, but not always, the recumbent. The introduction of a catheter, and retaining it in the bladder, if the urine pass through it ; rendering the urethra pervious, if this be occluded ; and the strictest attention to cleanliness, by frequent ablution and soothing vaginal injections, the constant application of a poultice made of goulard and bread crumb, and smearing the parts with an ointment consisting of equal parts of the ointment of the oxyd of zinc, and that of the acetate of lead well blended ; an ointment that I have found of great benefit as well in these exco-

riations, as in those of infants, the result of neglect or otherwise.

When the irritation is removed or lessened, then if the fistulous aperture be large, or not easily commanded, it may be reduced by touching the edges of it with the actual cautery, so as to enable the bladder to retain the urine longer, and after this has been attended to, a mould of softened wax* should be introduced into the vagina, so as to take accurately its form, a cast of this is to be taken in plaster of Paris, and in this is to be moulded a plug of caoutchouc, with or without an adapting stalk, such as we see in stalk pessaries, with a ball and socket joint, connecting it to a plate fitting to a vulva pad, all of which are to be kept *in situ* by a T bandage. [Doctor Kennedy here exhibited to the Society several of these instruments and plaster moulds of different forms.]

The following case was one so treated.

B. M., æt. 34, sent up in 1836 from County Meath, after her sixth pregnancy; all the children born alive but labours tedious; last followed by sloughing vagina, and with inability to retain urine. The vagina was found nearly occluded by the adhesion of its walls at one point, by which it was divided into two chambers, the adhesion occupying two-thirds of the circumference posteriorly, and on each side. The upper chamber engaged about one-fifth of the whole cavity, and had communication with the os uteri and bladder by a fistulous opening into its neck, about the size of a silver penny; the upper and lower chamber communicated by an aperture somewhat larger than a pea, through which the urine constantly escaped, as also the menses. Various methods were tried to relieve her, and she left hospital wearing a plug of plaster of Paris made to

* Dentists' wax should be obtained, and softened by exposing it to the heat of the fire, not by immersing in hot water; it should be rolled as nearly as possible into the shape of the vagina, and a strong tape, so placed within it as to catch the wax, and answer as a handle to assist in its extraction.

fit the vagina accurately, by first taking a cast of the part; this afforded her much comfort. she subsequently managed to be impregnated through this fistulous aperture, and returned to hospital to be delivered of her seventh child, in May 1838. Sloughing and peritoneal inflammation, however, succeeded upon her delivery, it having been necessary to divide the adhesions, and she sunk under them. [Dr. Kennedy, here exhibited the preparation of the parts and the vagina mould and plug to the society.]

Another case was that of Catherine Magrath, admitted to our diseases of female ward after her first labour. There is an opening into the bladder nearly the size of a halfpenny, the mucous coat protrudes on taking exercise or forcing down; vagina contracted superiorly; os uteri in situ seemingly fixed and patulous, about one inch of urethra anteriorly remaining pervious. The plug, contrived as before mentioned, was the only means found to afford relief.

Anne Home, æt. 24, fistula after first labour three years ago, vagina contracted by cicatrices, especially at upper part, os uteri cannot be satisfactorily ascertained. It seems as though the bladder and uterus opened into a common cavity of small extent, communicating inferiorly with the vagina, urethra permeable. A cast was taken and a gum elastic stopper made, which is supported by a bandage and pad, and thus she is enabled to retain her urine, which she passes at pleasure through the urethra without removing the plug.

The occlusion of the uterus may be complete, when the menses will of course be entirely retained; or it may be partially occluded and constricted, when the menses shall escape; in those cases where the contents of the bladder and menstruous secretion escape through a common opening, the greater part of the vagina being at the same time occluded, the menses would sometimes appear to pass through the bladder by a communication had with the upper chamber of the vagina; or the bladder and upper chamber of the vagina may have one common fistulous communication with the lower chamber, through which both urine and menses

pass out. In these cases, which are not unfrequent, we are precluded undertaking any means of completely closing the common aperture; as by preventing the escape of the urine we should do the same by the menses, and might cause them to accumulate in the bladder. We may however diminish the size of the fistulous aperture, so as to enable the patient to retain her urine a little longer; thus we did in the following case.

October 10th, 1838.—Anne Allen, æt. 26, married seven years, had five children, the last born in June of this year. The first three children were born by the natural efforts, but did not survive, the last two were extracted by instruments, according to her description, the crotchet. The day after the latter operation, which was performed, she says, after a labour of about forty-eight hours, she had frequent desire to make water, with pain in its expulsion; these symptoms continued for two months, when she lost the power of retaining it, unless when she lay quietly on her back. She menstruates regularly; on examination was found, adhesion of the walls of the vagina at its upper part, with a large opening which admits the finger into the neck of the bladder, and through it the os uteri can be felt projecting toward that viscus, and contained in a separate chamber. The fistula was touched with the actual cautery and diminished in size, but as she menstruated through it, it was not thought advisable to attempt to close it more completely.

In some of these cases of occlusion with fistulous communication with the bladder, the patients are able to retain their water for some hours; in such a case, the less we meddle with them the better, as their retaining power would appear to depend upon the excessive degree of constriction induced by cicatrization at the lower or occluded portion of the vagina, for instance.

Hanna Halpen, æt. 26, was delivered in the country, two years since, of her fourth child by the crotchet, after a tedious labour; on examination of vagina it is found firmly contracted, and at the distance of an inch from the external parts, the canal is stopped by a thick hard ring of the vagina, bounding an

opening into the bladder, and through which the menses pass. The urethra is impervious from an inch within the orifice; she suffers, however, but little inconvenience from the state of parts, as she can retain her urine for two or three hours at a time, and menstruates freely. The urethra was merely rendered pervious and she was discharged.

Now it will be seen in the above cases, that the extent of the treatment adopted was, removing existing irritation and excoriation; diminishing the aperture in some by the actual cautery; rendering the urethra pervious when obstructed, and adapting a mould* to accurately fit the parts and press upon the aperture through which the urine escaped; the minor inconvenience of the impervious state of the vagina, was put up with, when communication existed, admitting of the escape of the menses, as any attempt to remove it would only have added to the major evil, by rendering the patients less able to retain their urine. I wish this to be distinctly understood, as sometimes attempts are made to benefit these cases by operations, which by rendering the vagina more pervious, only add to the sufferings of the patient, leaving her totally unable to retain her urine; of course any operation directed towards the closing up the common menstruous and urinary aperture (even should it prove successful, which is very unlikely) would only create more serious mischief, by locking up the escape for the lochial discharge, which would, in all likelihood, accumulate in the bladder, and eventually a fresh aperture for its escape would become necessary.

* This may be constructed of any fit material. Dr. Churchill lately suggested to me in conversation, the trial of a simple method of making one of these plugs, by fastening a piece of sponge cut to fit the vagina, and covered with bladder or oil silk upon a stalk; it might also be used without the stalk.

ART. IX.—*Letter from DOCTOR MURPHY to DOCTOR STOKES,
on the Exhibition of Opium in Peritonitis.*

12, Upper Temple-street,
July 30th, 1839.

MY DEAR SIR,

At your kind suggestion, I have again perused your paper in the Dublin Journal, (vol. i.) on the Use of Opium in Peritonitis, and gladly take this opportunity of correcting the error I had fallen into, of attributing to you a priority in recommending it; I find you to state that “nine years ago (1823) Dr. Graves treated successfully two cases of peritonitis, after tapping, and occurring in patients of bad habits, *by opium*, without withdrawing a drop of blood, and more lately employed the same remedy in a case of peritonitis, from effusion of purulent matter into the serous sac.”

The error I had committed arose, from the length of time which elapsed since I had read your interesting paper rendering my recollection of its contents a little inaccurate; I would be obliged, therefore, if you would have inserted in the Dublin Journal this correction of my mistake.

I remain yours truly,

EDWARD W. MURPHY.

BIBLIOGRAPHIC NOTICES.

Medical Portrait Gallery; or Biographical Memoirs of the most celebrated Physicians, Surgeons, &c. By THOMAS JOSEPH PETTIGREW, F. R. S., &c. &c. Nos. 1 to 17.

WE can scarcely imagine a work calculated to be more useful to the profession than such a one as the present. It affords to the senior members a standard of comparison in the lives of their most distinguished predecessors, and gives the highest of all encouragement—that of example—to them in their noble, but laborious, and in many respects painful career. But to the young, to those commencing the practice of their profession, who are anxiously looking about for the right road to eminence, it is still more valuable, for from it they may gather an impartial direction as to the method to be adopted to ensure success. We think the account of the early struggles of distinguished men, the most instructive part of their lives; it is the period when the foundation is laid, and the materials accumulated for future celebrity; their after labours are at most the continuation of those exertions under favourable auspices, which were commenced in doubt, and difficulty, and discouragement. And most gratifying is the result of this investigation into the qualities which have deserved and attained success, though we could have wished that Mr. Pettigrew had given us more information on this point. It is true that here and there we meet with a character, whose intellect seems to have been his only recommendation; but upon the whole, we see that the men who have obtained the largest share of public favour, have been men whose early years were passed in study, in diligent research, in careful observation; whose intellect was improved by careful culture, whose industry was the offspring of principle—the cause, not the consequence of success—who were of strict moral and religious conduct, honourable in their intercourse with their professional brethren; gentle, and forbearing towards their patients, and benevolent to the poor. Many examples could be

given to prove this to be the case, and in the extracts we make we shall have this special object in view.

Let us turn first to the life of the great and good Boerhaave, and see the preparation for his future career.

“At the age of fourteen he was sent to the public school at Leyden, where he manifested extraordinary ability, and at sixteen was admitted into the University. He attended Suenguerd’s lectures in logic, natural philosophy, metaphysics, and ethics; and he so speedily obtained a proficiency in these studies, as to be able to maintain disputations in them. He studied rhetoric, chronology, and geography under Ryckius, and Hebrew and Chaldee under Trigland and Schaaf. He turned his attention to mathematics; and soon compassing geography and trigonometry, he studied algebra with Volder.”

The recommendation of John Vandenburg, that he should study physic, “determined his future course.”

“He appears to have laboured hard to acquire a solid basis for medical inquiry by a diligent attention to anatomy: he not only studied the works of Vesalius, Fallopius, and Bartolin; witnessed the dissections and demonstrations by Nuck; but he also most assiduously dissected, and visited even the slaughter houses, to make observations upon the structure of the different parts of various animals.” “The writings of the great father of physic, Hippocrates, he most carefully studied; because he found from a perusal of the Greek medical authors posterior to this writer, there was little of novelty to be found; but he did not altogether neglect the moderns, for in particular he esteemed the works of Sydenham, and expressly says, ‘that he frequently perused him, and always with greater eagerness.’ He made himself acquainted with chemistry and botany, and then went to the University of Harderwick, in Guelderland, where, in July, 1693, he took the degree of Doctor in Physic.”

This was laying a good foundation; and it did not require the gift of prophecy to foretell that such a man would find a way to distinction, or make one. The stepping stone to eminence with him was his appointment to a lectureship, which gave him an opportunity of using profitably the stores of information he had so laboriously acquired. That he succeeded beyond what any of the present day can expect, is notorious. But his virtues equalled his intellectual attainments.

“He was sincere and devout, and is a strong example to oppose to the aspersion *religio medici opprobrium medicorum*. Under very acute sufferings he exhibited great patience and submission. In his friendships he is described as sincere, constant, and affectionate; as a man, communicative without conceitedness, dispassionate in contending for truth, and averse from censure. In his manners he is

reputed to have been of great innocence and simplicity, entirely free from moroseness, which so frequently manifests itself in those who have conversed more with books than men. No less meritorious was his conduct in the administration of justice in the course of his rectorship; no respect of persons could make him deviate from rectitude; no power of the great could awe him into compliance with any unworthy purpose; 'he was modest without meanness, and steady without rudeness.'"

Let us next turn to the life of one who was deservedly esteemed in this city, one whose retirement was opposed most strenuously by those who might have expected to have benefited from it. Dr. Cheyne was born on the 3rd of February, 1777, at Leith, and the medical profession may be almost said to have been his hereditary estate. His father, grandfather, grand-uncle, and great grandfather practised physic in Scotland. After a good elementary education, he studied medicine for the sake of a diploma, which having obtained, he was appointed assistant surgeon to a corps of artillery. Whilst holding this situation he led an idle life, and was more intent upon amusement than improvement. This season of dissipation, however, could not long satisfy a mind like Dr. Cheyne's; he became "anxious to distinguish himself in his profession," and to secure an honourable station among his brethren.

"He therefore, in 1799, left the horse artillery and returned to Scotland, resolved once more to become a medical student. On his return he was appointed to the charge of the Ordnance Hospital at Leith, and he undertook to act as his father's assistant, whose practice, especially amongst the poor, was very extensive. He selected for study the best cases from those which fell to his lot in a division of the business of the day. These cases he journalized; and when he foresaw that a disease would end unfavourably, he took measures to ensure a necroscopic examination. At this period he happily formed a friendship with Mr., now Sir Charles Bell, who, then particularly occupied with the study of pathology, was laying the sure foundation of the highest professional eminence. Sir Charles opened most of the bodies which Dr. Cheyne obtained permission to dissect, taught him many things which he might not otherwise have learned, and confirmed his taste for distinction."

Having determined to commence practice in some large city, Dr. Cheyne set himself in good earnest to obtain a thorough acquaintance with those classes of disease which came under his observation, and especially with certain epidemics, acute diseases, and diseases of children.

"When a well marked case of disease occurred, or when an epidemic arose, he obtained the best monograph he could on the subject,

and attentively compared the information which it contained with the opinions of the most experienced of his professional brethren, whom he had frequent opportunities of meeting, and then he filled up his case books; thus by means of reading, observation, and the experience of others, his mind was made up on the most important points of practice, and with decision he acquired a facility of prescription in acute diseases, which proved of great advantage to him, especially in dispensary practice. With respect to chronic diseases, in addition to the assistance he derived from books and observation, he obtained aid from a mass of consultations, many of them written with great care, by the most eminent physicians in Edinburgh, during the middle and end of the eighteenth century, which had been preserved by his grand uncle, father, and grandfather Edmonston."

It would scarcely be possible to chalk out a better plan as a preparation for successful and extensive practice than this one pursued so diligently by Dr. Cheyne; nor can we be surprized at the confidence reposed in his judgment, now that we see how well disciplined his mind was in professional detail. But even thus early Dr. Cheyne's penetration taught him, that more than a mere knowledge of medicine was essential to professional success, and

"He endeavoured to become acquainted with the character of those who had obtained the highest rank in the profession of medicine in Edinburgh, in order to discover the causes of their success; and he ascertained, that although a man might acquire popularity by various means, he could not reckon upon preserving public favour, unless he possessed the respect of his own profession; that if he would effectually guard his own interest, he must in the first place attend to the interest of others; hence he was led carefully to study, and liberally to construe, that part of medical ethics, which regulates the conduct of physicians towards each other."

In the year 1809, Dr. Cheyne settled in Dublin, and though for some time he shared the lot of all junior practitioners, having received from the 9th November, 1810, to 4th May, 1811, only three guineas; yet from this obscurity he soon emerged. In 1811 he was appointed Physician to the Meath Hospital, and Lecturer in the Practice of Physic in the School of the College of Surgeons; in 1815 Physician to the House of Industry, and in 1820 Physician General; and as his practice then yielded him about £5,000 per annum, (which was its annual average during ten years,) he felt that he had fully attained the object of his ambition. We have now seen the preparation of mind for successful practice and its natural consequences, public confidence, extensive reputation, and large emolument. We cannot conclude without noticing the high testimony which was

borne to his character in the address presented to him by his professional brethren on his retirement from the fatigues of practice. We had marked several extracts from the lives of other celebrated men ; but these must be deferred for the present. For much valuable instruction and pleasing information we must refer our readers to the work itself, which, we hope, will soon be in the library of every practitioner, and within the reach of every student. It is "gotten up" in very elegant style, and what might be deficient in the descriptions is compensated for by the engravings, many of which are very good. We trust Mr. Pettigrew will excuse us for recommending the addition of a few more Irishmen to his list of "distinguished individuals." It would not be easy to equal the number or the character of those Ireland has produced ; and many there are of whom all we can now say is, that long may it be before they become appropriate subjects for such a biography.

Researches on the Development, Structures, and Diseases of the Teeth. By ALEXANDER NASMYTH, F. L. S., F. G. S., &c. &c.

THIS is the first part of an elaborate treatise upon the teeth, and is occupied with a history of the progress of knowledge on the subject. We have no hesitation in saying, that if the subsequent parts shew equal care, research, and ability, the work will be the standard book on the subject. We could extract many interesting passages descriptive of the additional information contributed by each physiologist in succession ; but our object is rather to communicate to our readers the abstract given of the more recent discoveries of Purkinje, Retzius, Owen, &c. It is impossible, however, to pass over the labours of Löwenhoeck, Hunter, Serres, &c. In the year 1678 the former published a paper in the Philosophical Transactions, containing the following remarkable passage :

"I have some time since applied a glass, (esteemed by several gentlemen who had tried it, a very good one,) to observe the structure of the teeth and other bones, which, both to them and myself also, then seemed to consist of globules ; but since then, having drawn out one of my teeth, and for further observation applied better glasses than the former, the same gentlemen with myself agreed, from what we plainly saw, that the whole tooth was made up of very small, straight, and transparent pipes. Six or seven hundred of these pipes put together, I judge, exceed not the thickness of one hair of a man's head. In the teeth of a cow the same pipes appear much bigger, and in those of a haddock much less."

Similar information is given in his *Continuatio Epistolarum*, and he further shews his acquaintance with the cortical substance, “undiquâque alio esse circumducti erant; adeo ut jam me adhuc magis quam antea certum reddere possem, circum primo confectum dentem os accrevisse.”

He likewise describes crystals found in the cavities of horses' teeth; but these observations attracted little attention at the time.

John Hunter published his work on the teeth in 1778, which, to a clear statement of the then state of knowledge, contained the results of his own experiments.

“He says that the enamel, which he calls *lamina vitrea*, is composed of striæ running from the circumference to the centre; he believes that it is quite inorganic, because it cannot be converted, by any means whatever, into animal mucus: exposed to fire it separates from the ivory. He calls the latter the osseous part of the teeth, and holds that it is analogous to bone. He concludes from his experiments, that the teeth are to be considered as anomalous bodies with respect to their circulation. He thinks that the roots are surrounded by a periosteum, which comes from the alveolus, and which is prolonged into the cavity of the tooth; and that the ivory is formed of concentric laminae. He states that the incisors have three centres of ossification; the canine only one, and the molars three or four: that the tooth, at the moment of extrusion, is a foreign body in relation to the gums; and that the enamel is probably secreted by the capsule which surrounds the body of the tooth before it is extruded.”

Bichat and Blandin followed up these views with their characteristic acuteness; nor have our own countrymen been idle, as the works of Blake, Fox, and Bell amply testify.

M. Serres' work published in 1817 contains valuable information, especially relating to their organization at an early period. He says,

“The rudiments of the head, and of all the organs which it contains, exist almost as soon as the embryo begins to be distinct in the product of conception; the teeth, which are destined to perform the first function in the circle of life, are formed and developed in the interior of the maxillary bones. Preceding anatomists have only ascertained the existence of their germs at a late period of foetal life; but I have thought it necessary to examine them at as early a period as possible. In the jaws of an embryo of two months, I found the germs of the first incisors and small molars: but I could not find the canine teeth till two months and a half; at three months I discovered the germs not only of the first, but also of the second dentition, and even that of the wise tooth. These germs are found lodged in membranous folds, which form at this period the gum; those of the first

dentition are immediately attached to this fold; those of the second are suspended from it by a pedicle of about two lines long, which alone enables them to be distinguished, together with the yellow colour which they contract by exposure to the air, and which contrasts with the dull white of the gum. At four months, I found fibrous partitions separating the incisors, but all the other germs were contiguous to each other. At six months, the osseous partition of the incisors was very distinct; that of the small molar was also partly ossified, and the two posterior molars were contained in the same division."

Some beautiful observations upon the dental membrane, arteries, and nerves, are given from Serres' work; but our limits oblige us to omit them, as well as the discoveries of Cuvier, Rousseau, Hildebrand, &c.

Purkinje's experiments preceded those of Retzius, and were, in many respects, confirmed by him and Müller. As respects the enamel our author observes:

"According to Purkinje's researches the enamel consists of simple perpendicularly placed fibres, increasing somewhat in size towards their upper extremity, forming quadrilateral prisms, and often making several curves on one and the same level. These fibres are adherent to lamellæ, which run transversely round the tooth, and generally rest obliquely on the surface of the dental substance."

We shall now quote a portion of the account given of Retzius' discoveries:*

"He first examined the cartilage in teeth which had been macerated in diluted muriatic acid, and found, in making sections of it, that it appeared to consist of slightly undulating fibres lying close together, resting with one extremity against the cavity of the pulp, whilst the other terminated near the surface of the tooth. It was not till a subsequent period, he says, that he discovered that these were hollow; but in the mean time he observed no internal similarity between this cartilage and that of bone. He next procured, with the aid of a file, slices of tooth bone as thin as writing paper; increased their transparency by means of olive oil and turpentine varnish; and on examining them under the microscope, found that they were composed not merely of hollow fibres, but of ramifying tubes, of which the trunks opened into the cavity of the tooth, supporting themselves on its pulp, whilst their terminations, in the form of extremely fine branches, run towards the external surface of the tooth. The branches were most distinct, and most easily shewn at the external extremities of the main tubes; but in the whole course of the canals both dictio-

* His work is entitled "*Mikroskopiska Undersökningar öfver Tandernes Sardeles Tandbenets structur.*" Stockholm, 1837.

tomous divisions and delicate branches were observed ; the latter were infinitely minute, and formed, as it were, a peculiar vascular system. Under reflected light and on a dark ground all these tubes seemed to contain a white matter.

“ The enamel he found to consist of hexagonal solid prisms, on which were generally observed transverse lines or streaks, making them appear as if they were formed of several pieces, lying one above the other : they rested with one end against the crown of the tooth, and with the other formed a part of its surface.

“ Round the roots and necks of teeth he found a structure presenting some similarity to osseous substance, from the small cavities it contained, which, under reflected light, appeared to be filled with white matter ; and from the number of winding, anastomosing tubes, as well as other larger canals, opening into these cavities. This is the cortical substance which has been described as existing in the teeth of elephants, pigs, and oxen, by Tenon, G. Cuvier, and F. Cuvier ; but which plainly forms a part also of the teeth of man and many other animals, in which it has not hitherto been remarked.” (pp. 49, 50.)

The course of the contiguous tubes is beautifully described ; but for this we must refer the reader to the volume itself, as we are anxious to quote a few more extracts concerning the enamel and cortical substance.

“ Retzius states that the enamel is adherent internally to a thin membrane, which, he continues, is probably the remains of that surrounding the organ for the production of the enamel, which has been described in such a masterly manner by Purkinje. This membrane resists for a long time the action of water. In teeth which had been macerated for several months in water, he has found it remaining, on his separating and dissolving the enamel in acids. To his great astonishment, when he once dissolved in dilute muriatic acid a long slice of enamel from the fossil tooth of a horse, (which had been dug out of a turf moor,) he found, after the solution had been effected, this membrane floating at the top of the fluid. He examined it under a glass of considerable magnifying power, and found it perforated in every part, but it shewed no trace of fibres.” (p. 56.)

“ When recently formed enamel is submitted to the action of diluted muriatic acid, every delicate prism leaves behind on its solution a small portion of organic matter : but as this cannot be formed at a later period, it is probably only a deposit from the moisture, which, in the first place, surrounded the enamel fibres.

“ The enamel fibres appear under the microscope as small angular needles, about $\frac{1}{500}$ of a line P. M. in diameter. On many of them are small crowded transverse streaks, of which some run round the entire fibre, some only round a part of it. The enamel fibres rest with one extremity on the membrane mentioned above, which is pressed closely on the dental bone. A glass which magnifies three hundred times

is necessary, in order that the enamel fibres may be distinctly observed; they are then seen ranged side by side, like the wax tubes in a honeycomb, and like them they are hexagonal." (p. 59.)

All will confess, we are certain, the great interest of these minute researches, and they who seek will find much more to reward them. Let us examine now the *cortical substance*, which,

"Like dental and other bone consists of cartilage and osseous earth. From the cartilage of human teeth, of which the osseous earth has been dissolved in acid, the cartilage of the cortical substance may be separated in the form of a membrane, which is thickest at the end of the root: in human teeth it appears to have less consistence than the cartilage of dental bone. Viewed under the microscope, it presents the same cellules, or so called corpuscula, as the proper osseous substance, dental bone, and most cartilaginous structures. It was less easily dissolved in boiling water than that of dental bone, and retained several earthy granules after those of the latter had all disappeared. When either recent or dried cortical substance, polished or in fine slices, is examined with a good lens, a number of white crowded points, almost invisible to the naked eye, are discerned in it, which, on being further magnified, shew themselves to be the cells above mentioned, deriving their white colour from the osseous matter they contain. As in dental and common bone, numerous tubes pass into and from them, widening as they enter, and giving them the appearance of irregular stars. These tubes have numerous communications with each other, partly direct and partly by means of branches $\frac{1}{100}$ th or $\frac{1}{50}$ th of a line P. M. in diameter: some of them pass immediately from one cell to another, precisely as in the dental bone. The osseous cells are of various forms and sizes; some are elongated, so as to look almost like tubes; some are nearly round: their average size Ritzius found to be $\frac{1}{450}$ th of a line P. M. In sections made transversely to the axis of the tooth, it is plainly seen that the osseous cells are arranged in parallel lines or concentric rings, some fainter than others, or that the cortical substance is deposited in delicate coherent layers. In the human tooth the cortical substance is an extremely thin stratum, taking its origin in most teeth with a complete root at the neck, where the enamel terminates, and increasing in size as it descends towards the extremity of the root, where it is generally thickest. In young teeth, of which the roots are not yet fully developed, it is so thin that the osseous cells in it are not perceptible; and it has merely the appearance of a delicate membrane. On the other hand, the older a tooth is, and the more the *cavitas pulpæ* is closed, the thicker is the cortical substance at the end of the root, where it sometimes forms a considerable enlargement, or what is commonly called an *exostoses*. Retzius has seen a case where this increment was of the length of three lines at the root; and has also examined a wise tooth, when the cortical sub-

stance was so plentiful at the neck, that it formed a sharp projecting border turned towards the crown, and gave to the whole tooth an uncouth appearance. In teeth projecting considerably from the alveolus, and which have not been worn away, Retzius found it sometimes thicker than the dental bone itself; and this he thinks is particularly the case in those teeth which have been loosened by the scurvy or by the use of mercury." (p. 63.)

We have preferred giving these lengthy extracts, rather than an abstract of them; because the discoveries of Retzius are better exhibited thus, and also because our readers will be better able to appreciate the able manner in which Mr. Nasmyth has performed his task. We hope at a future time to enter more fully into the diversity of opinions on the structure of the teeth, and to do full justice to the labour of our own countrymen; but for this we wait the appearance of the second part of this work, where Mr. Nasmyth's own researches will be given. It is almost superfluous to say that we recommend this little volume to our readers. The style is simple and unaffected, the language clear, the abstracts of different opinions just and able, and the plates admirably executed. We take leave of Mr. Nasmyth, with a just respect for his talents and industry, and wish to meet him again before long.

A Treatise on the Diseases of the Heart and Great Vessels, &c.
By J. HOPE, M. D., F. R. S., &c. &c. 3rd Edition.

DR. HOPE is favourably known to the profession as a laborious and ingenious investigator of the diseases of the heart, and that his labours have been to a great extent appreciated, the appearance of a third edition clearly indicates. That his industry has not flagged is manifested by the increased size of the volume, and the additional information therein contained. It will be in the recollection of our readers that we reviewed this work on its first appearance, and that we deemed it our duty to criticise it somewhat severely. It is much to Dr. Hope's credit that our suggestions have been attended to. We have not now to complain of no notice having been taken of certain contemporary physiologists, though we think they are disposed of rather cavalierly. We cannot agree with Dr. Hope's theory of the heart's sounds, but we do full justice to the ingenuity and patience with which he has investigated them; and we think many of his practical observations well worthy of the attention of the practitioner.

As this is our second notice of this work, we do not deem

it necessary to enter into any further analysis. We gladly acknowledge that in many respects this edition is an improvement of the former one, and whilst we differ in some points from the learned author, it gives us pleasure to see his continued zeal in his favourite pursuit. The style is clear and intelligible, and the arrangement lucid. We are sure that few practitioners will be without the work, embracing as it does an immense mass of important facts and a considerable extent of original observation.

Medical Notes and Reflections. By HENRY HOLLAND, M.D., F. R. S., &c., Fellow of the Royal College of Physicians, and Physician Extraordinary to the Queen.

DR. HOLLAND is a physician of considerable note in London, and is already favourably known to the literary world by former publications. The work before us is both instructive and amusing, and contains the germs of many improvements in the practice of medicine: its object will be best understood by a perusal of the preface.

“The title of this volume is chosen as being that which most nearly expresses its contents. Though appearing now as detached papers, they are founded chiefly upon notes made in the course of twenty years of medical practice in London. During nearly the whole of this time, I have been accustomed to preserve notices, not merely of particular cases, but also of such general reflections as were suggested to me by actual observation. At the expiration of the period named, I have thought it well to look back upon these various memoranda; to give something of more definite form to those which seemed worth preserving; and to compare the whole, as well with my own present impressions, as with the actual state of knowledge on the several subjects in question. This volume is the result of a revision and selection so made. But as its form, though sanctioned by precedents of high value, is not altogether common, it may be right to add a few words more, in explanation of the motive and manner of publication.

“It seemed to me, looking at them as impartially as I was able to do, that there were, among these papers so revised, a certain number which might be likely to contribute in some degree to medical knowledge, or to the exactness of our views in practice. I have had regard to these objects only, as justifying publication; and as a principle of selection out of the materials before me. In making this selection, I have put aside a great deal as relating to subjects of inferior importance; still more, from finding that many of my notes related to facts or opinions no longer new. What I have retained includes much that will be familiar to all who have carried observation and study into their medical life. But this was inevitable,

without wholly omitting many subjects of great interest; and it would have been presumptuous to offer a work composed, as this has been, from materials acquired in the course of active practice, as one of original research.

“Every physician has, in his progress, some particular occasions or facilities capable of being converted to good. The opportunities of private practice, from which almost exclusively my own observations are drawn, do not furnish the same striking conclusions as those of hospitals; nor the large classes of facts, which form the statistics of medicine, and are so fruitful of results. Yet such is the scope of the subject, that a prolonged experience, with due regard to the nature and sufficiency of the evidence, may from this more limited source derive much to aid other methods of research, and to enlarge the general amount of medical knowledge.

“What I would fain hope may be found in this volume is, a just view of some of the relations of diseases, as well to each other as to the healthy functions of the body; the correction of some doubtful or erroneous views in practice; suggestions which may be useful as regards particular classes of remedies; and reflections on certain points of physiology, in which, without any pretension to experimental inquiry, it appeared to me that something might be gained by arranging the facts and inferences in a new form. On topics of the latter class I have sought especially to associate pathology with physiology, the morbid with the natural and healthy states of the body; believing this principle of modern inquiry to be above all others fertile in sound conclusions, and far from being yet worked out to its full extent. If I might venture on giving any distinctive character to the volume, it would be that of aiming throughout at this object.

“In one instance only have I indulged in any mere speculation; and this only interrogatively, as to an old hypothesis, regarded in its relation to modern science, and to the history of a remarkable disease. Many other points I have put as questions; finding them as such in my notes; and thinking it well, whenever it could be done, thus to mark the objects most open to inquiry. I have further taken the privilege (which more than twenty years’ experience may perhaps be allowed to sanction) of commenting on certain usages and details of practice, in which the character and usefulness of the profession, and, through them, the welfare of the public, are materially concerned.

“On each subject treated of, I have brought together my notes in the manner best suited for perspicuity; adding whatever seemed necessary to give greater completeness to the reasoning, or to connect it with the inquiries of others on the same topics.* In effect of

* On the latter point great deficiencies are inevitable. In these days of various research, actively pursued in so many countries, it would require more reading than is compatible with actual practice to collect together all that has been done on these subjects; nor would a single volume suffice for the mere reference needful to such a work.

this, much of the volume has been wholly written anew. I had at one time proposed the insertion of a greater number of the cases upon which its materials were founded: but I abandoned the intention, from a wish not to increase the size of the work, which this must largely have done. And for the same reason I have abridged into the form of notes many topics on which I had originally written at greater length.

“As respects the arrangement of the subjects, it will be found a very desultory one; such as naturally arose out of the miscellaneous materials employed. In a few instances only have I thought it worth while to bring topics expressly together for their mutual elucidation. To have attempted this further would imply a more complete and consecutive work than that now offered to the public.

“These, I think, are all the circumstances which need be stated in explanation of the form and matter of the volume. It will deeply gratify me, if hereafter I find cause to believe that it has contributed to science by any right views in the philosophy of medicine, or to practical good by any suggestions of value regarding the treatment of disease.”

The volume is an octavo, with remarkably good print and paper, containing 628 pages; it is divided into thirty-five chapters, with the following headings:

On Medical Evidence.—On Hereditary Disease.—Bleeding in Affections of the Brain.—On Sudorific Medicines.—Effects of Mental Attention on Bodily Organs.—On Points where a Patient may judge for himself.—On the Connexion of certain Diseases.—On the Abuse of Purgative Medicines.—On Methods of Prescription.—On Gout, and the Use of Colchicum.—On some supposed Disease of the Spine.—On the Brain as a double Organ.—On some Points in the Pathology of the Colon.—On the Epidemic Influenzas of late Years.—On Dreaming, Insanity, Intoxication, &c.—On Mercurial Medicines.—On the Exercise of Respiration.—Method of Inquiry as to Contagion.—On the Medical Treatment of Old Age.—On the Use of Emetics.—On the Uses of Diluents.—On Morbid Actions of intermittent Kind.—On Diet, and Disorders of Digestion.—On Diseases commonly occurring but once in Life.—On the present Questions regarding Vaccination.—On the Use of Opiates.—On Sleep.—On the Influence of Weather in relation to Disease.—On Time as an Element in Mental Functions.—On Phrenology.—On disturbed Balance of Circulation, and Metastasis of Disease.—On the Use of Digitalis.—On Antimonial Medicines.—On the Hypothesis of Insect Life as a Cause of Disease.—On the present State of Inquiry into the Nervous System.

We subjoin the whole of the twentieth chapter, which we

have selected as one of the shortest, and therefore best suited to our narrow limits.

“ Among the changes which time and fashion impose upon the treatment of disease, emetics as a remedy have fallen into comparative disuse in English practice. It is no longer now, as heretofore, one of the first questions before the mind of the practitioner, when called to prescribe for a disorder, whether the stomach should not be thus immediately relieved. The suggestion of the patient himself is for the most part needed, or some other equally express indication to lead to their use. I am speaking here only of a general alteration that has occurred. The exceptions to it are of course numerous; but the truth of the fact so stated will probably not be denied.

“ The cause of this change may chiefly be found in that larger and more various employment of purgatives which forms the character of our modern practice. To what extent is the alteration of method a beneficial one? I believe a fair consideration will justify the opinion that in many points it has been carried too far. Vomiting, especially when brought on at the outset of fevers and many other diseases, produces effects of a kind, and with a speed, which no purgatives alone can equally obtain. Beneficial as is the action of emetics in unloading the stomach and upper part of the alimentary canal, it is certain that their influence goes far beyond this; and that other parts of the body, even the most remote and different in structure, are powerfully acted upon. The emetic moreover, if rightly used, is beneficial not merely in the effect of morbid matters removed from the body, but often even by the nausea attending its operation, and by the mechanical effort of vomiting; a combination of advantages scarcely belonging to any other remedy.

“ The experiments of Magendie seem to have proved, that the stomach itself is passive in vomiting; and that the action is due to convulsive movements of the diaphragm or abdominal muscles, brought on, not solely by emetic or other irritating substances in the stomach, but by similar agents introduced directly into the blood, as well as by certain states of the brain and nervous system. As I have in view only the practical application of the remedy, I need not advert to these points in physiology, further than to remark, that they illustrate much more satisfactorily the various causes of sickness, and the effects of emetics, than the old notion that the stomach alone was concerned in this action. There are still points of question as to the proximate causes of vomiting; even such as relate to the state of the diaphragm in the act, and in what particular way it is connected with the function of respiration. Something also may be learned of the manner in which particular irritations produce sickness, by reference to reflex nervous actions through the spinal cord. But, for the reason just given, I merely allude to these topics of inquiry.

“ It is unnecessary to say much of the effects of vomiting as a remedy upon the stomach itself. The relief got by rejecting undi-

gested food, or morbid ingesta of other kinds, is not, however, to be regarded as the only benefit thus obtained. The secretions from the mucous membrane lining this organ are frequently such in kind and quantity, that their removal becomes necessary to all further treatment; and accordingly we find that instant good is often derived from emetics, where these matters alone are discharged. While still left on the stomach, all other treatment by internal remedies is injurious or unavailing; a point not sufficiently regarded in ordinary practice, where the failure of one medicine is too often made the prelude to another of similar kind. It may be that emetics have influence also in changing the nature of the secretions from this organ, either by inducing the state of nausea, or by the act of vomiting; each, as it would seem, capable of this effect, though perhaps in different manner.

“Much caution, however, is necessary as to the use of emetics in another state of the stomach and alimentary canal, where the secretions from the lining membranes are almost wholly suspended; and where there is general gastric irritation, tending to inflammatory action, indicated by redness and dryness of the tongue, fauces, and throat, and often by tenderness over the region of the stomach itself. These, indeed, are cases where the effort to vomit is often a symptom, and a very distressing one, of the disorder; and where emetics of course are interdicted by the most obvious circumstances present.

“The value of emetics in the treatment of cynanche tonsillaris, though understood as matter of medical precept, is not equally regarded in practice. Nor, indeed, as a general fact, do we sufficiently keep in view the close relation between the several parts of the internal fauces, and the membranes of the œsophagus and stomach; and the frequent and singular translations of morbid action which take place along this continuity of surface. Many more exact interpretations of disease might be derived from looking to this connexion through the medium of contiguous structure and function, than from that minute nomenclature which is formed upon the mere locality of symptoms: and the indications of treatment would in the same proportion become more exact, as in the use of emetics under the circumstances to which I am now referring.

“The liver, and the whole system of the portal circulation, are singularly under the beneficial influence of this remedy. Congestion, so prone to occur in this part of the body at the commencement of most fevers, as well as in other diseases, is lessened or removed by vomiting,—the passage of bile into the bowels rendered more free,—other secretions promoted or restored; purging often obtained without further aid, and a general state of all these viscera induced, peculiarly tending to lessen febrile action, and especially in those cases where it arises directly from gastric disorder.

“In most of the slighter cases of jaundice with which we have to deal, emetics will be found much more capable of speedily restoring the passage of bile into the bowel than any other means; and the

instances are numerous (including particularly those fevers of warm climates, which, however variously designated, have all such close relation to the functions of the liver and the circulation through the chylopoietic viscera) where the direct combination of an active emetic and purgative medicine in the outset of disease, with repetition of the same means when needful, has effect in subduing the violence of disorder beyond any other remedy. This combination is not sufficiently employed in ordinary practice, in the many cases where speedy disgorgement is required of the part of the system just referred to.

“The effect of emetics upon the circulation is a complicated one, including, as it does, both nausea and the mechanical effect of vomiting; each capable of influencing greatly the heart’s action, and the general movements of the blood. From both, however, may such benefit be obtained; though with different application according to the nature of the case. When the circulation is morbidly excited, nausea may be longer sustained; where there is congestion about the heart and great vessels, vomiting is speedily to be brought on. Nor is there cause for the degree of apprehension which exists as to the rupture of blood-vessels, or pressure upon the head, from this effort. Though cases of such kind have doubtless occurred, they are comparatively very rare, and generally depend upon causes peculiar to the individual. It is singular indeed to what extent, and with what violence, vomiting may take place, without producing any injury of this nature. Sea-sickness, and that attending every stage of pregnancy, are evidence of the fact; and it is important the inference should be kept in mind, that there may not be undue discouragement to the use of the remedy in many cases where it is of singular value.*

“The influence of emetics upon the nervous system is also complicated, and one in which it is not easy to separate the direct from the indirect effects. It is unquestionably powerful, and capable of being very beneficially applied. The circumstances, not yet wholly understood, which give such peculiar sensibility to the region of the stomach, and so closely associate it, not only with all others of the vital organs, but also with the functions of animal life, render the several conditions of this part very important to the well-being of the whole system. No illustration of this is needful to those who have felt the influence, both upon mind and body, of the sensations arising from disorders having this seat, even such as are casual and tempo-

* “I may mention in further proof the common use of emetics among the workmen in coal-mines, for the relief of the state brought on by breathing fixed air in excess, as well as after being subjected to fire-damp. I have had recent occasion of learning this fact in visiting the mines at Whitehaven. It might be supposed in these cases, if in any, that congestion in the vessels of the brain was to be avoided. Yet if injury be thus produced, it is seemingly of much less import than the good gained by renewing the action of the diaphragm and restoring respiration.”

rary in kind.* Here the action of emetics is well marked, and often of singular avail; and from hence their effects are largely diffused over other parts of the nervous system. Some part of the good they produce, when given at the outset of fevers, may depend on this circumstance, in concurrence with the others already mentioned.

“ In illustration of the benefit to be obtained from emetics in this stage of disease, I would refer again to the epidemic influenzas, which have so repeatedly prevailed of late years. I have already stated my conviction that no remedy is equally safe and expedient, at the commencement of the disorder, in diminishing the severity of the attack; or apparently, when slight, in suspending it altogether. In the progress of the malady, too, there is often benefit from the repetition of the same means; and especially in the case of children, where the want of due relief by expectoration is best thus supplied. I may name this as an instance where the comparative disuse of emetics has certainly interfered with the general success of our practice.

“ The mention of emetics as aiding expectoration brings us to one of the most important uses of this remedy. Though yet insufficiently employed for the express object, their effect in relieving the chest, when the bronchial cells and tubes are gorged with mucus, is scarcely less beneficial than their action in unloading the biliary system. They may justly be reckoned the most powerful expectorants we possess; speedy in effect, and often complete in relief. In bronchitis and other cases, where from accumulation of mucus in the air passages, the breathing and pulmonary circulation are greatly oppressed, and the patient under much suffering, the change thus produced is sometimes surprising in degree, and such as we can obtain in no other way. Their value in croup is well attested by the concurrence of all modern experience with that of the excellent physician who first enforced this treatment.

“ I have already commented on the apprehension of hæmorrhage, or pressure on the head, which checks one important application of emetics. In the case now before us, their use is often abandoned from the like fear of making undue pressure on the vessels or producing suffocation; an apprehension unwarranted by fact, and which the most common experience might correct. The converse of this is nearer the truth, and forms, indeed, the especial value of the remedy in the instances alluded to. Even in cases of actual inflammation of the lungs I believe the risk of their use to be exaggerated, though undoubtedly more watchfulness is here required.

“ There is the greater cause for referring to this application of eme-

* “ The fitness of the term of *foyer epigastrique*, as expressing a fact respecting this particular part of the body, has been made the subject of some controversy. Bichat denies its propriety as applied to any one spot; but, in connexion with his hypothesis, that the vital organs are the proper and sole seat of the passions, (an hypothesis which has foundation in language alone,) he makes a sort of admission of the phrase in question, as expressing a central influence of these organs.”

tics from our vague and imperfect views as to the whole class of expectorant remedies. Scarcely indeed is the term defined in its ordinary use in practice. It is left doubtful whether the expectorant is a medicine which promotes the secretion of mucus from the bronchial surfaces, or facilitates, after being formed, its removal from the chest; or combines both these effects in its single power. The natural result is that of rendering practice almost equally vague on these points. And, though there is less liability to dangerous error here than in many other classes of remedies, yet it is obviously important to gain more distinct views than those currently received.

“The emetic is probably the only agent which both promotes secretion and discharges it; the latter action being chiefly, if not altogether, a mechanical effect of the effort of vomiting induced. There is reason to believe that no one of the medicines termed expectorants can act in freeing the chest from mucus in the air passages, unless they be so given as to produce vomiting, or to bring on cough by irritating the membranes; or unless they increase or attenuate the actual secretion, so as in this manner to excite cough, and render it more effectual for expectoration. Their influence upon secretion is indeed the circumstance we must chiefly regard among medicines of this class; and here, again, their effects and relative value are very ill defined. It is probable that the expectorants, so termed, which act as emetics when given in larger doses, are principally of avail in augmenting the secretion when used so as to keep up a certain degree of nausea. This state has manifestly much influence in relaxing the exhalant and secreting vessels; and though I do not venture to affirm it, where proof is so difficult, I believe that it is chiefly in this way that the medicines in question come to be of any avail in practice.*

“The question as to the proximate cause or seat of the sensation which we term nausea, and its relation to the act of vomiting, is indeed a curious and difficult one. If any thing like explanation is to be found, it will probably be through the results obtained by Magendie and others, to which I have already alluded.

“Connected with this subject is the alleged effect of emetics as a remedy in the early stages of pulmonary consumption; an opinion held by many eminent physicians from an early period down to the present day; and which has gained rather than lost weight by recent inquiries on the subject. The researches of Dr. Carswell into the origin and seat of tuberculous deposits afford a more explicit notion how emetics may act, by removing or preventing the growth of tubercle on the membrane of the bronchial cells. It is easy to understand that any means which can promote the natural secretion into these

* “The whole class of expectorants, however divided and defined, needs revision, as do so many other parts of the *Materia Medica*. The progress of medicine, as a science, requires that we should not bind ourselves too implicitly to old tables and formulæ, which have their origin in doubtful sources, and gain authority chiefly by long transmission from one book or lecture to another.”

cells, render their contents more liquid and easy of removal, and aid in actually procuring it, may be of singular advantage; especially in that early part of the disease, where the presumption exists that tuberculous deposits are only beginning to take place. The action of emetics reaches further towards these several objects than that of any other remedy; it is compatible with every other part of treatment, and under regulation of their use, and with due regard to any acute inflammatory states which may occur in the progress of the disease, I believe them to be the safest and most effectual means yet suggested for the relief of incipient phthisis.

“Unfortunately this must still be stated more on speculation than on certain experience. A few continental physicians have made systematic use of the remedy; but in England its employment with this object seems to have been very partial, though enforced by medical authorities which claim every respect. The difficulties, indeed, which oppose themselves to the treatment, especially among the higher classes, are not easily overcome. The remedy, from various associations, is regarded as more formidable than is really the case; the present habit of practice is adverse to it; and, further, the suggestion of the treatment comes at a time when fears may hardly yet be awakened, or when there is repugnance on the part of the patient and those around, to admit what argues a dangerous disease at hand. The influence of these causes is well known to every physician.

“The use of emetics in producing absorption of effused fluids, or of parts morbidly enlarged, seems sufficiently attested. But we have no reason to suppose that they have any effect on tubercles actually formed; and their employment therefore in phthisis, when we can obtain trial for them at all, is probably to be limited to the earliest stage of the disorder.

“I need advert but slightly to the benefit derived from emetics in asthma; though here again it must be admitted that there is an insufficient use of the remedy, seeing the great good gained in many such cases by unloading the stomach and liver; and the equal advantage, though less obvious in explanation, from its influence on the actions of the circulation and nervous system. A single emetic may cut short a paroxysm for which opiates and antispasmodics have long been employed in vain.

“The use of this remedy in the disorders of children is at present much less general than it ought to be. In very many cases emetics would beneficially supersede that employment of purgatives which often adds to the irritation it professes to remove. In the infantile fever, for example, which is a type of various disorders, an occasional dose of ipecacuanha, so as to excite vomiting, (especially where there is much of the cough which attends this complaint, and large secretion from the mucous membranes,) will be found more effectual than any other means. It is to be noted, further, that the action of vomiting is for the most part singularly easy in children; more immediate, and generally less distressing than that of purgative medicines.

“In these remarks I have attended rather to the general effects of emetics, than to the several qualities and manner of action of the medicines so termed; it being my object only to draw attention to the fitness of their larger and more defined use in ordinary practice. The question of preference among different emetic substances is indeed of less moment, from their action in emptying the stomach of its contents. In the majority of cases, that may be deemed best which fulfils its purpose without actual pain, and with greatest certainty and speed. If ipecacuanha were invariably of good quality, which unfortunately it is not, it might be sufficient in almost every instance. Antimonials, from sustaining nausea longer, and producing more distinct sedative effect on the nervous and vascular systems, may be preferred where excitement of these exists. The emetic of simple mustard ought never to be lost sight of as an immediate resource, and one producing its effect with less previous distress than any other.

“In considering, however, the effects of emetics, we must separate such as belong to the direct action of the medicine on the coats of the stomach, from those produced by the act of vomiting. The former may be more or less hurtful, as depending upon a peculiar irritation of the part; and here the action of emetic medicines must be assimilated to that of acrid or poisonous matters received into the stomach or generated there. But the mere effort of vomiting itself is much less injurious to this organ than might on first view appear likely. Without referring in proof to the “*vomitum luxuriæ causâ*”* of the Roman dinner tables, I may again mention sea-sickness, the sickness of pregnancy, the frequent vomiting of infants, and the habit some individuals have of rejecting constantly in this way a portion of all food taken, as evidence that little mischief comparatively can be derived from this source.† The point is one which must always be kept in view in judging of the effect of emetics in practice, and of the frequency and particular methods in which they may best be employed.”

* “Celsus, lib. i. cap. 3.

† “I have known a patient, a young lady of delicate and irritable habit, who, during fourteen months, rejected invariably some portion of every meal; the quantity rejected, and the interval before vomiting occurred, depending much on the quality of the food. In this case (in which the malady depended not on organic disease of the stomach or pylorus, but seemingly on irritation translated from another part) the patient suffered chiefly from an uneasy distention preceding vomiting, little from nausea or the vomiting itself; and she even gained considerably in flesh while the disorder was going on.

“In another instance I have known the habit of vomiting continue for many years, after every meal of which animal food was the principal part; without any apparent injury to the constitution or increasing mischief to the stomach. In cases of this kind, the act of raising the rejected food is generally attended with very little nausea or distressing effort.”

SCIENTIFIC INTELLIGENCE.

First Annual Report of the Registrar-General of Births, Deaths, and Marriages in England.—We are much indebted to T. H. Lister, Esq., the Registrar-General, for an early copy of the above work, which he forwarded, immediately on its publication, to the Editors of this Journal. All who have watched the progress of medical science must be aware of the immense importance of the system, lately instituted by the Legislature, for the purpose of ascertaining the facts necessary for the cultivation of medical statistics in the most extended sense of the term.

“Long before the commencement of the registration,” (says Mr. Lister,) “my attention was turned towards an object admitted to be of great importance to the improvement of medical science, namely, to obtain a faithful statement of the *cause of death* in the column of the register set apart for that purpose. It is obvious that such statements, in order to obtain due credit, ought to be derived, whenever it is possible, from the medical attendant of the deceased person; and that therefore it was only from the co-operation and assistance of the practising members of the medical profession that a satisfactory attainment of this important object could be expected. I, therefore, in the spring of 1837, communicated on this subject with the President and Fellows of the Royal College of Physicians, the President and Members of the Royal College of Surgeons, and the Master and Members of the Society of Apothecaries, and obtained the concurrence of those bodies, signified by the signatures of their respective Presidents and Master, appended to a joint circular address, pledging themselves, in every instance which may fall under their care, to give an authentic name of the fatal disease, entreating all authorized practitioners throughout the country to follow their example, and to assist in establishing a better registration, and inviting them for that purpose to attend to a subjoined explanatory statement, wherein they would see set forth the provisions of the recent Statute, and the means whereby the important object so recommended might most effectually be obtained. The ‘explanatory statement’ which I had submitted to these corporate bodies, and which thus received their combined approval, after stating who are the persons who, according to the act for registering births, deaths, and marriages in England, must give information to the

Registrar, on being requested so to do, ‘earnestly recommended that every practising member of any branch of the medical profession who may have been present at the death, or in attendance during the last illness of any person, shall, immediately after such death, place in the hands of such other persons as were in attendance, of the occupier of the house in which the death occurred, and of some inmate who may probably be required to give information, written statements of the cause of death, which such persons may show to the Registrar, and give as their information on that subject.’ Having done this, I caused copies of the circular address, with the subjoined explanatory statement, to be sent to all authorized practitioners of medicine and surgery throughout England and Wales, whose names and addresses I was able to obtain. I have the satisfaction of stating, that the measures thus adopted have been eminently successful, and that even in this first year of registration—the commencement of a new system—the result is such as will not disappoint the expectations of those who hope to derive, eventually, from that source, materials of vast importance to the advancement of the science of vital statistics.

“In order that these materials should be available for such beneficial purposes, it was obviously essential that they should be duly arranged. The execution of this task, for the first and second quarters, has been committed by me to Mr. Farr, a gentleman of the medical profession, whose scientific knowledge and intimate acquaintance with statistical inquiries were ample pledges of his peculiar fitness, and whose letter, including abstracts of the causes of death, and explaining the classification which has been adopted, I have the honour to lay before your lordship in the Appendix to this Report.”

The following observations by the Registrar-General are of great importance :—

“In the abstract of deaths, (the registration of which, even for this first year, has been effected with signal success,) I have entered into more minute details exhibiting enumerations of the deaths of persons of each sex at every successive year of age. Such details are of acknowledged value, as data for determining the laws of mortality, as bases for calculations materially affecting the interests of millions. Tables exhibiting the proportion of deaths at every successive year of age, are among the most important materials from which are deduced the true principles on which should be founded the systems of life annuities and of life insurance, and the rules of friendly societies established for the use of the poorer classes. The materials hitherto accessible are admitted to have been too limited for framing, satisfactorily, tables to regulate the amount of contribution at various ages, by which members of such societies may become entitled to allowances in old age, or to sums payable at death. The insufficiency of the data hitherto collected, and the contradictory nature of the several tables founded on them, are strongly set forth in the Report of the Select Committee of the House of Commons, in 1827, on the laws respecting friendly societies. It is there stated that ‘according to the Northampton tables, out of 1,000 persons existing at the age of twenty-five, there survive at the age of sixty-five, 343 persons.

By the Carlisle tables, no fewer than five hundred and thirteen persons will survive ;' whereby it appears ' that a society which should adopt the Northampton tables would, if the mortality among its members should correspond with the Carlisle tables, have *three* annuitants where it calculated upon *two*. Of those annuitants moreover, a large proportion would live to enjoy the annuity for a considerable number of years ; for instance, of the three hundred and forty-three persons, who would be annuitants according to the Northampton table, ninety-eight would live for fifteen years ; according to the Carlisle tables, one hundred and sixty-two persons would survive through that period and attain the age of eighty years.' But still more clearly will it appear how great is the want of further facts for the elucidation of these important subjects, and the establishment of a safe standard, by viewing in a tabular form a comparison of the various results of seven approved tables of mortality, which I subjoin in a note (*see next page*), extracted from the above-mentioned Report. The recommendation of that Report, that measures be adopted for making ' an accurate and extensive collection of facts,' whereby may be facilitated ' the solution of all questions depending upon the duration of human life,' is at length carried into effect ; ample materials, thus conducing to ameliorate the condition of the working classes, are now afforded in the certified copies of registers deposited in the General Register Office ; and each year's accumulation will increase the value of such records, by augmenting the number of facts upon which calculation may be brought to bear.

“ In pursuance of these objects, I have felt that it was of great importance not only to give an abstract for the whole kingdom of England and Wales, but to exhibit the difference which prevails in different portions of the kingdom ; to compare town with country—agricultural districts with manufacturing and mining districts—the hilly with the low and level—the maritime with the inland—the eastern and northern with the western and southern parts. Nor are these diversities matters of merely curious speculation, but may be made the source of important benefits, especially to the poorer classes. It was stated in evidence before the Committee on Parochial Registration, in 1833, by the Actuary of the National Debt Office, that the extent of difference which then existed was utterly unknown—that tables for the use of the poor in reference to sickness and mortality, and in reference to the regulation of their Friendly Societies, could not then be constructed for two districts differing in character, from the want of such information as an improved system would afford ; and that if two societies of poor men, residing in different districts of a totally different character, were at the same time, to apply to him for tables to guide them in preserving their societies solvent, he ' should be under the necessity of giving the same tables to both, though knowing perfectly that the rates which were adequate in one case were inadequate in the other.' It was also stated to the Committee on Laws respecting Friendly Societies, by another eminent actuary (Mr. Milne,) that no one table or scale of contributions can, with propriety, be adopted by all friendly societies ; that one composed of members living in or

near a manufacturing town, required a table very different from that which would be required in places where the population is less dense, and where a considerable proportion of the members are chiefly employed in the open air; but that these are differences which he could not 'pretend to estimate for want of *data*.' The useful principle of comparison may, if requisite, be carried out into a more minute system of subdivision than I have, in this first instance, deemed it necessary to adopt. But there was danger lest, in attempting a more subtle discrimination, we should lose sight of broad distinctions which it was important to observe; and it was necessary to remember, that to diminish by subdivision the number of facts on which calculation could be brought to bear was materially to diminish their value. The extent to which division should be carried is a question not to be decided by any established rule, and which necessarily admits of much diversity of opinion; and I have endeavoured (not unaided by judgments which I respect) to pursue a middle course between the opposite extremes of subdivision and condensation, dividing the kingdom into the twenty-five portions, in which are exhibited abstracts of deaths at different ages. In doing this I have, as will be seen, had regard not so much to the observance of established boundaries as to those circumstances from which diversity may be expected to arise; and I have, in some instances, included in the same table contiguous counties similar in soil, climate, elevation, and the employments of the people, and have, in other instances, disregarded the boundary of the county where it was desirable to compare two large portions of its inhabitants pursuing very different occupations.

	By Dr. Price's Table, founded on the Register of Births and Burials at Northampton.	By the first Swedish Tables, as published by Dr. Price, for both Sexes.	By Mr. De Parcieux's Table, founded on the Mortality in the French Tontines, prior to 1745.	By Mr. Milne's Table, founded on Mortality observed at Carlisle.	By Mr. Griffith Davies' Table, founded on the Experience of the Equitable Life Insurance Office.	By Mr. Finlaison's Tables, founded on the Experience of the Government Life Annuity-tants.	
						According to his <i>first</i> investigation, as mentioned in his evidence in 1825.	According to his <i>second</i> investigation, as mentioned in his evidence in 1827.
						Mean of both Sexes.	Mean of both Sexes.
Of 100,000 persons aged 25, there would be alive at the age of 65	34,286	43,137	51,033	51,335	49,330	53,470	53,950
Of 100,000 persons aged 65, there would be alive at the age of 80	28,738	23,704	29,873	31,577	37,267	38,655	37,355
Expectation of life at the age of 25 years	30.85	34.58	37.17	37.86	37.45	38.35	38.52
Expectation of life at the age of 65 years	10.88	10.10	11.25	11.79	12.35	12.81	12.50

Towards the conclusion of the Report, Mr. Lister remarks :—

“ Among the diversities which especially demand attention, and by which there is least danger of being led to false conclusions, are those which relate to longevity, showing the varying proportions of deaths in old age in different portions of the kingdom. From a few instances of extreme longevity no inference can be safely drawn ; but the fact that of the deaths in any district a comparatively large proportion is above the age of seventy, is a strong presumption in favour of the health of that district. These proportions will be found to vary greatly. In the whole of England and Wales, out of one thousand deaths, 145 have been at the age of seventy and upwards ; while in the North Riding and northern part of the West Riding of Yorkshire, and in Durham, except the mining districts, the proportion has been as high as 210. In Northumberland (excluding the mining district) Cumberland, Westmoreland, and the north of Lancashire, the proportion has been 198, in Norfolk and Suffolk 196 ; in Devonshire 192, and in Cornwall 188.

“ In contrast with this evidence of the large proportion of persons who attain to old age in these more thinly-peopled portions of the kingdom, we find results extremely different where the population is densely congregated. In the metropolis and its suburbs the proportion who have died at 70 and upwards has been only 104 ; and even this proportion is favourable when compared with that of other large towns—the proportion in Birmingham being 81, in Leeds 79, and in Liverpool and Manchester only about 63.

“ A comparison of the mining parts of Staffordshire and Shropshire, and of Northumberland and Durham, with the rural districts surrounding each, exhibiting great differences, especially in the proportion of deaths in old age, will justify the distinctions which I have made in placing them in separate tables.

“ A very marked diversity also appears in the proportion of deaths of infants in different parts of the country. In the mining parts of Staffordshire and Shropshire, in Leeds and its suburbs, and in Cambridgeshire, Huntingdonshire, and the lowland parts of Lincolnshire, the deaths of infants under one year have been more than 270 out of 1,000 deaths at all ages ; while in the northern counties of England, in Wiltshire, Dorsetshire and Devonshire, in Herefordshire and Monmouthshire, and in Wales, the deaths at that age out of 1000 at all ages scarcely exceeded 180.

“ But it is less essential that I should point out diversities which examination of the appended table will easily detect, than that I should advert to other circumstances which must also be borne in mind by those who would avoid erroneous inferences. The comparative table alone will not accurately indicate the comparative risk of death at different ages, in different divisions of the kingdom, unless it be true that in each division there is the same proportion of persons living of the same age. But such an assumption is utterly disproved by the enumeration of the ages of persons living in May, 1821, made under the authority of the Population Act, which shows a wide

difference in the proportions which were then found in the several counties.

“ To illustrate the risk of error from inattention to that circumstance, I will compare the deaths between the ages of 20 and 50 in division 1, comprising the metropolis, with those in division 6, comprising Herefordshire, Bedfordshire, and Buckinghamshire, and the rural parts of Middlesex. It appears that in division 1, the deaths between those ages, out of 1000 at all ages, are 241, while in division 6, they are only 192; from whence arises apparently an undeniable inference that there is some cause operating in the metropolis tending to cut short life when it has run through about half its natural course, which does not equally affect it in the adjacent rural districts. But it appears from the enumeration made in May, 1821, that of 10,000 persons of both sexes then living in Middlesex, there were 4,522 between the ages of 20 and 50; while the mean number of persons of the same age in Bedfordshire, Buckinghamshire, and Hertfordshire was only 3,581, a remarkable difference, attributable to the circumstance of persons born in the counties near London quitting them, as they emerge from childhood, to seek employment in the capital. There were thus 4,522 persons exposed to the risk of death, between the ages of 20 and 50, in Middlesex, for 3,581, in those three adjacent counties; and if the mortality were equal, the deaths between those ages in Middlesex, and in the other three counties, ought to bear the same proportion. Therefore, upon the supposition that the proportions of the living remain the same as in 1821, the results exhibited by the abstract of deaths will not be unfavourable to the metropolis.

“ Again, on comparing the proportion of deaths under 5 years of age in division 19, (comprising Lancashire, south of Morecambe Bay, with the exception of Manchester and Liverpool,) with the same in division 25, comprising Herefordshire, Monmouthshire, and Wales, it appears that in the former division there were 458 deaths and in the latter 365. But on referring to the table, founded on the enumeration of 1821, it appears that out of 10,000 persons of each sex in the county of Lancaster exposed to the risk of death, under the age of 5, there were 1,711 males, and 1,582 females; while in Hereford the proportion was only 1,317 males, 1,326 females; in Monmouthshire, 1,320 males, 1,404 females; in Wales (collectively) 1,514 males, 1,382 females. If, therefore, the proportion of the living at the above-mentioned age continued to be the same in the year ending June, 1838, and the mortality at that early age were the same in both divisions, the proportion of deaths in division 19 would of necessity be greater than in division 25.”

The letter of Mr. Farr to the Registrar-General contains a very able and accurate analysis of returns from different parts of England, bearing on the relative mortality and diseases of different towns and districts. The results are so important that we think it right to lay them before our readers, premising that in these returns diseases are divided into two great classes, epidemic and sporadic. The epidemic class includes the following, small-pox, &c. :

“ STATISTICAL NOSOLOGY.

FATAL DISEASES.

Synonymes, Provincial terms, &c.

Epidemic, Endemic, and Contagious Diseases.	SMALL-POX . . .	Variola.	} Swine pox, water pox, hives. (1)
	Chicken pox . . .	Varicella.	
	MEASLES . . .	Rubeola.	
	SCARLATINA . . .	Rosalia. Scarlet fever.	}
	Putrid sore throat .	Cynanche maligna.	
		Ulcerated sore throat.	
	CROUP	Cynanche Trachealis.	
	HOOPING-COUGH .	Pertussis. Chin-cough.	
	THRUSH	Aphthæ. Flox, frox, frost, white mouth.	
	DIARRHŒA	Looseness, purging, bowel complaint.	
	DYSENTERY	Dysentery. Flux, bloody flux.	
	CHOLERA	Cholera biliosa. Cholera infantum.	}
	Asiatic cholera . .	Cholera epidemica.	
	INFLUENZA	Catarrhus epidemicus.	
	ERYSIPELAS . . .	St. Anthony's fire.	
	Puerperal fever .	Under childbed.	
	Mumps	Cynanche parotidea.	} Low fever, jail fever.
	TYPHUS*	Febris typhoides.	
	Synochus	Inflammatory fever.	
	Miliaria	Miliary fever.	} Brain fever.
	Gastric fever . . .	Febris gastrica.	
	Nervous fever . . .	Febris nervosa.	
	Bilious fever . . .	Febris biliosa.	
	Remittent fever . .	Febris remittens.	
	Yellow fever . . .	Typhus icterodes.	
	Plague	Pestis	
	SYPHILIS	Lues venerea.	
	HYDROPHOBIA.”		

All other diseases are included in the class sporadic.

“ The deficiencies in the first half-year are far from considerable ; and it appears that the registration of the causes of death, notwithstanding its imperfections, does not yield in accuracy to any similar statements which have been made public. Still, as statistical observations gain weight by numbers and time, it must not be concealed that the results deducible from the half-year's registration are limited, in comparison with the results which will flow naturally from a more extensive series of facts ; and I shall therefore confine the tabular combinations, and the inferences to which they lead, within such limits as leave little chance of error.†

* “ Mumps, miliary fever, remittent fever, yellow fever, and plague, are so rarely fatal in England, that they have only been distinguished in the notes ; and are referred in the abstract, with all the modified species of fever, to typhus, as the number of fatal cases thus confounded with typhus is too inconsiderable to affect the resulting mortality. Scorbutus, purpura, scrofula, purulent ophthalmia, rheumatic fever, pellagra, cretinism, elephantiasis, Barbadoes leg, beribery, yaws, leprosy, malignant pustule, ergotism, hospital gangrene, when they occur in England, appear only to occur sporadically.”

† “ It may be right to add, that the Carlisle table was founded upon 1840 deaths.”

“Influenza prevailed epidemically in the beginning of the year 1837, and destroyed great numbers ; it subsided in March, and the rest of the year was considered healthy. The number of burials collected, and published by the London parish clerks in the months of January and February (or rather 20th December, 1836, to 21st February, 1837), were 2,053, and 2,336, while the average monthly burials of the next nine months were 1,355.*

“In order to deduce from Table A, an approximation to the rate of mortality by each separate cause of death in the half year ending 31st December, 1837, it will be requisite to obtain an approximation to the population of England and Wales on the 1st October, 1837. As the population has been enumerated four times at decennial intervals, and been found to increase very uniformly, I have assumed that the annual rate of increase was the same in the six years five months, from 31st May, 1831, (the time of the last census,) to the 1st October, 1837, as in the ten years 1821-1831 ; and that the proportion of males and females was the same as in 1831. This calculation gives as the total number living, 1st October, 1837, 15,268,056 ; or 7,439,123 males, 7,828,933 females. A certain number of the males has, however, never been enumerated. The correction for this should not embrace the whole of the army, the navy, or the migratory English population, in the colonies, or on the continent, but solely the proportion of the population, who if they died, would be registered in this country. The correction for the deficiency in the enumeration of males has been assumed to be 1.67 per cent. ; thus making the proportion of males living to females as 10,000 : 10,351, instead of 10,000 : 10,625, as it is directly given by the census.

“Another correction is required for the deficiencies in the registry of the first half-year. The death is registered two or three days after it occurs, and as nearly 1,000 deaths are registered daily, 2,000 or 3,000 of the deaths which happened in the first quarter were necessarily registered in the second ; 2,000 or 3,000, which happened in the second quarter, were registered in the third quarter, &c. ; and thus the numbers belonging to the first compensate the second quarter for the deaths which, happening in the second, were registered in the third quarter. But the first quarter has no compensation, and a certain number must be added as a correction to the 148,701 deaths registered in the first half-year. The causes of death were not specified in 7,094 instances. In framing the next table, I have, therefore,

* “It may not be uninteresting to compare the results of the registration with the London bills of mortality. The total burials in the bills of 1837 amounted to 21,063 ; and in the six months, 21st June to 26th December, 1837, corresponding nearly with the half year during which the Registration Act had been in operation, the number of burials in the bills was 10,518, (*Gentleman's Magazine*, 1837.) The number of deaths registered in the same parishes under the Act was 18,266 ; and as 4,450 burials, many of which occurred in the first half of the year, are set down to December, it may be safely asserted that the parish clerks registered little more than half the deaths that occurred within the limits of the London bills of mortality.”

added, as a correction for these and other omissions, 10 per cent. to the causes of death specified; or rather diminished by 10 per cent. the population, which serves for the divisor. The deaths occurred in half a year. To obtain the annual rate of mortality the population was further divided by 2, leaving 3,438,228, and 3,558,605 as the divisors of the two columns in Table A, headed 'Males,' 'Females.' The annual rate of mortality per cent. was then found to be nearly—

Males.	Females.	Mean of the two Sexes.
2.08	1.97	2.02 per cent.
or, 1 in 48	1 in 51	1 in 49

(See TABLE B.)

"The mortality was comparatively low in this half year; in which the weather was mild, and no destructive epidemics prevailed extensively. It agreed nearly with the annual mortality of the seven years 1818-24; estimated by Mr. Edmonds at 2.03 per cent.*

"The total number of deaths registered amounted to 148,701, of which 75,159 were of males, 73,542 of females. The cause of death was assigned in 141,607 instances, leaving 7,094, or 4.8 per cent. unaccounted for; but it must be added that natural death, and all vague unintelligible terms have been referred to the latter head. It has been assumed in the subsequent tables, that the deaths in which the fatal diseases were not registered, had the same causes as those in which the causes of death were registered; and this was the nearest approximation that could be made to the truth.

"32,537 deaths were occasioned by the first class of diseases: and it will be seen in Table B, that the males suffered more than females in the proportion of 4.7 to 4.6 per 1000 annually: small-pox, croup, thrush, diarrhoea, dysentery, and cholera, having proved more fatal to males than females; while influenza and hooping cough, particularly the latter, cut off a greater number of females; and typhus, erysipelas, scarlatina, and measles, affected both sexes equally.

"Of 4.651 in 1,000, the mean epidemic mortality of the two sexes, 3,036, occurred principally among children, as small-pox, measles, scarlatina, hooping-cough, croup, thrush, diarrhoea; although a considerable number of adults were also carried off by these diseases. Cholera, dysentery, influenza, ague, typhus, erysipelas, and syphilis, constituting the remaining 1.615 per 1,000, attacked adults chiefly, although they did not entirely spare children.

"Measles destroyed 4,732 lives, scarlatina 2,520, hooping-cough 3,044, diarrhoea 2,755: but neither influenza nor cholera was extensively epidemic in this period; and small-pox and typhus were the reigning maladies.

"Small-pox destroyed 5,811 lives in the half year. It will be seen subsequently that small-pox was epidemic in several parts of the country, particularly in Liverpool, Bath, and Exeter. The following were the ages at which 1,056 of the deaths occurred in Bath,

* "Medical Annual, 1833."

Liverpool, Exeter, parts of Shropshire, Worcestershire, and the metropolis.

“ Deaths from Small-pox.

Age .	0-4.	5-9.	10-14.	15-19.	20-29.	30-39.	40-49.	50-59.	All ages.
Deaths	887	99	15	18	29	5	2	1	1,056

“ This specification of the ages may be considered an approximation to the ages of the 5,811. It has a direct bearing on a very important practical question which has recently engaged attention, namely, whether the prevalence of small-pox is due to the diminished influence of vaccination, or whether the protective power of vaccination progressively declines, so that a child vaccinated at two years of age is more susceptible of small-pox at the age of 20, or 30, than at any earlier age. The facts in the registers would decide the question if the medical attendant ascertained whether the individuals, who died of small-pox had ever been vaccinated, and if this fact and the interval between vaccination and death were entered : which might easily be done. In the mean time it seems exceedingly probable that the majority of the 5,811 had never been vaccinated, as they were very young, and when the poorer classes do not neglect vaccination altogether, they often defer it for years. Vaccination is delayed too long by all classes ; it should not be practised later than the first three months, as the early deaths at Bath and Liverpool testify.

MONTHS.					YEARS.											
Age	0—2.	3—5.	6—8.	9—11.	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.	Above 10.	
Bath	5	7	12	9	33	31	33	17	17	6	4	..	2	..	10	
Liverpool . .	17	34	42	50	143	127	77	64	24	19	11	4	2	3	25	
	22	41	54	59	176	158	110	81	41	25	15	4	4	3	35	

“ One infant aged ten days, died of small-pox ; three were only a fortnight old.
“ The small-pox was raging in Liverpool when the registration began, and of 495 deaths from that disease, 129 were registered in July, 127 in August, 110 in September, 78 in October, 32 in November, and 19 in December. The epidemic was apparently declining in Bath. Of 151 who died of small-pox, 71 died in July, 60 in August, 20 in September. 36 of the cases occurred in the Abbey district,

36 in the Lansdowne district, 27 in Walcot, 25 in Lyncome and Widcome, and only 7 in Bathwick, 11 in Batheaston, 9 in Tiverton. The epidemic was commencing in Exeter, where 21 died of small-pox in July, 47 in August, 64 in September; and of these 77 in St. David's district, 55 in St. Sidwell.

"A remarkable epidemic dysentery prevailed at Taunton, in Somersetshire. It had been previously observed in the Bridgewater Union, and began in Taunton in July, when two deaths were registered. Greater numbers were attacked, and of 206 cases of dysentery, 16 of diarrhœa, and 1 of cholera, 75 were registered in August, 105 in September, 29 in October, 10 in November, and 2 in December. 114 of the deaths occurred in the district of St. Mary Magdalen, 69 in St. James's, 24 in Pitminster, 13 in North Corry, 3 in Bishop's Lydeard. The epidemic spread partially amongst the surrounding unions.

"Ages of 223 who died of Dysentery, Diarrhœa, and Cholera in Taunton.

Age	0-4.	5-9.	10-14.	15-19.	20-29.	30-39.	40-49.	50-59.	60-69.	70-79.	80-89.	90—.
Deaths	93	17	11	7	6	3	7	16	26	24	11	2

"In Table M is given a list of the principal epidemics and the districts in which they prevailed.

"Under sporadic diseases of the nervous system, 21,852 deaths (15 per cent. of the total number) were registered. The mean annual rate of mortality from the whole group was 3·1 per 1,000; but males suffered more than females in the proportion of 3·4 to 2·8. Paralysis, chorea, and epilepsy, were the only maladies which affected more females than males. Cephalitis, hydrocephalus, and convulsions—the diseases chiefly but not exclusively incidental to young children—cut off 2·4 males and 2·0 females out of 1,000 living; apoplexy, ·42 males, ·35 females; tetanus, ·013 males, ·003 females; delirium tremens, ·025 males, ·002 females. Tetanus generally follows wounds, and is therefore remotely caused by accidental violence, to which males are more exposed than females. Delirium tremens is also sometimes brought on by wounds in drunkards, and in persons exhausted by passion or misery. The insane who die in lunatic asylums have often been registered improperly under secondary diseases, such as apoplexy and diarrhœa. A considerable number of the sudden deaths, returned visitation of God, natural death, &c., at inquests, are apoplexies; but the proportion is unknown. Convulsion comprises a great proportion of very young infants. It is a frequent termination of all infantile diseases, and probably includes several diseases undeveloped, or the result of diseases of the epidemic class, for it is subject to great variations in different places.

"The notes mention some singular causes of death under this head.

Ten deaths—nine of females one of a male—are ascribed to mental emotions of one kind or other; seven to fright, one to grief for the death of a son, and two to a broken heart. It is well established that grief and distress are the roots of various organic diseases, and many cases are recorded in which sudden mental shocks have destroyed life, or induced madness. Where life is destroyed instantly, the connexion of the shock with the death is less equivocal than in cases where a considerable interval elapses between the two events. It happens that in two of the cases the alleged cause, as well as the deaths, were recorded. A female aged 63, it is said, died November 7th from trouble for the death of her son; and from another page of the register it appears that the son's death occurred October 30th. Again the death of a female, aged 41, on November 14th, is ascribed to 'fright,' occasioned by the sudden death of her brother; and her brother died on the 19th of October.

" 38,522 deaths were attributed to diseases of the respiratory organs, or 27 per cent. of the total number of deaths. The mortality of males and females was the same—5·5 per 1,000 annually. Bronchitis, pleurisy, pneumonia, hydrothorax, and asthma, destroyed more males than females out of the same number living; consumption and decline more females than males in the ratio of 4·155 to 3·771. Decline comprises a few cases of atrophy, and organic diseases of the intestinal canal; but the majority were evidently cases of true consumption—tubercular phthisis, to which some of the cases under hæmorrhage, improperly designated 'rupture of a blood vessel,' should also be referred. The deaths from this dreadful malady amounted to 27,754—20 per cent. of the total number of deaths; or nearly 4 annually, out of 1,000 living. Pneumonia, which it must be recollected includes 'inflammation of the chest,' was next in fatality to phthisis; but young children furnished the majority of the cases: of 379 fatal cases of pneumonia in the metropolis, and in some county districts, 228 were children under three years of age. Several of the cases were also evidently the sequelæ of hooping cough and measles.

" Under the fourth class, 1,596 deaths were registered. The mortality of pericarditis was the same in both sexes; aneurism was nearly three times as fatal in males as in females; and of the entire class the mortality of males and females was as ·262 to ·195 per 1,000. The proportion of deaths ascribed to heart diseases is much below that stated by Dr. Clendinning, and much below the truth. A great number of the dropsies and sudden deaths, and apoplexies, are the effects of hypertrophy and valvular diseases. Rheumatism is rarely fatal, except when it seizes on the heart. Angina pectoris was met with thirteen times; six times in males, seven times in females. The heart was ruptured in three males.

" 5,115 males and 4,735 females died of disease of the digestive organs, and the annual rate of mortality in the males was 1·5, in the females 1·3, out of 1,000 living. Nearly all diseases of the intestinal canal proved most fatal to males, so did jaundice and all the diseases of the liver, except hepatitis. Hernia is much more common in males than

females, yet the mortality in the two sexes—·044 and ·029 per 1,000—did not differ so widely, and the reason of this is, that the bowel, when it does escape, is much more liable to strangulation in the hernia of females than in the hernia of males.

“ In a classification purely topographical, the deaths from thrush, diarrhœa, dysentery, and cholera, should be added to the diseases of the digestive organs; which would raise the mean annual mortality of the class from 1·41 to 2·07, or if typhus were also added, to 3·36 per 1000.

“ No cases of painters’ colic was distinguished. Constipation comprises deaths by intussusception, 11 males, 10 females; lieus, 3 males, 3 females; stricture of the œsophagus, 8 males, 22 females; stricture of the rectum, bowels, and pylorus, 7 males, 6 females. The stricture would be produced in many cases by cancerous diseases. Thirty cases of hæmatemesis were registered, 18 males, 12 females.

“ Diseases of the urinary organs destroy five times as many males as females—the rate of mortality of the two sexes, under this head, having been ·199 and ·037 per 1,000. The disparity has been ascribed to mechanical causes; but will a mechanical explanation account for the fact that 68 males and only 27 females died of diabetes? Dr. Yelloly, in a paper published in the Philosophical Transactions, estimated that 1 in 108,000 persons was cut annually for stone in England and Wales.* It appears from the table that 47 in 1,000,000 males, and 5 in 1,000,000 females die of stone and gravel. The latter, it must be admitted, is a vague term in popular language; but the mortality from stone is certainly 1 in 100,000 annually. Bright’s disease is registered ‘Disease of the Kidneys.’ The coagulability of the urine is often undetected by careless practitioners. A female child, aged two years, is stated to have died *suddenly* of diabetes.

“ 1,265 females died in childbed, and the greater proportion of puerperal fever. According to an estimate by Dr. Ferguson, this insidious disease occasions ‘seven-eighths of the total mortality in childbed.’† The annual rate of mortality by childbirth in females, is 3·55 in 10,000, or in females, at a child-bearing age, perhaps 0·8 per 1,000. If the number of births and miscarriages in the period was 290,000, nearly 4 in 1000 were fatal to the mothers. It is greatly to be regretted that in the present state of medical science 2,500 women die in childbirth every year in England and Wales. It will be seen subsequently that the chance of death is nearly doubled or diminished one-half in certain circumstances.

“ One female died of extra-uterine fetation; one of hæmorrhage from rupture of the fallopian tube; one of apoplexy in childbirth; one aged 20, of childbed and rheumatic fever.

* “Remarks on the Tendency to Calculous Disorders, by J. Yelloly, M. D., Transactions of Royal Society, 1829, page 1-55.”

† “Essays on the most important Diseases of Women.”

“ Although the bones, muscles, ligaments, and tendons, constitute so much of the bulk of the body, the diseases of the entire motive system scarcely prove more fatal than the diseases of the kidneys and bladder; including rheumatism, the mortality is $\cdot 000133$. The bones are, notwithstanding, very liable to mechanical injuries, and several of the violent deaths are the result of fractures. Lumbar abscess and diseases of the joints are the principal items under the head ‘disease.’

“ 264 deaths occur under diseases of the skin, and of these twenty-three were of purpura hæmorrhagica; seven of leprosy. Thirteen cases of scurvy should have been classed under the next division, had the term not evidently been used several times in the popular sense—scurvy—a scurfy cutaneous disease, and applied to very young children. Carbuncle and fistula are most common in males; fourteen males, five females having died of the former, thirty-nine males, twelve females of the latter disease. In a topographical arrangement, small-pox, measles, scarlatina, and erysipelas, should be added to diseases of the integumentary system. In that case the mortality would be $1\cdot 976$ per 1000, and it is a curious coincidence that the mortality from all the diseases of the digestive organs is nearly the same— $2\cdot 067$. The diseases of the connected internal and external membranes are equally destructive; but the mortality from diseases of the two systems together is only 4, while that from diseases of the respiratory organs is $5\cdot 5$ per 1,000.

“ 16,207 diseases of indeterminate seat were registered; and the mortality of the two sexes from the entire class was almost the same— $2\cdot 310$ and $2\cdot 322$ annually out of 1,000 living; yet in some diseases there were great differences in the two sexes. Hæmorrhage—the effusion of blood—was the most frequent in males, in the proportion of $\cdot 107$ and $\cdot 060$; dropsy—the effusion of serum—was the most fatal in females, $\cdot 882$ females and $\cdot 711$ males having died of dropsy out of 1,000 living. 14,105 deaths were ascribed to inflammations; the annual rate of mortality of males was $\cdot 00218$, of females $\cdot 00186$, mean $\cdot 00202$; one-tenth of the fatal diseases were inflammations of one part or other. In deaths from abscess, mortification, and scrofula the males exceed the females. With the next in the class the reverse happens, 873 females and 355 males having died of carcinoma. Tumour should probably be added to carcinoma in many instances; the proportion of the two sexes was fourteen males, twenty-three females. Debility was composed chiefly of premature or weakly infants; and it is well known that male infants and the male fœtus die in greater numbers than individuals of the other sex. Malformation is also more common in males than in females: the males, whose deaths were caused by malformation, amounted to seventy-five, the females to forty-one. Males are, according to the registers, more intemperate than females, as the deaths of seventy males and of fifteen females were ascribed directly to intemperance; of sixty-seven males and twelve females to gout; of eighty six males and nine females to delirium tremens (in the second class.) It will be seen with

regret that in the half-year the deaths of sixty-three individuals were ascribed (principally at inquests) to starvation; this is almost one annually to a population of 111,000. The want of food implies the want of every thing else, except water—as firing, clothing, every convenience, every necessary of life, is abandoned at the imperious bidding of hunger. Hunger destroys a much higher proportion than is indicated by the registers in this and in every other country; but its effects, like the effects of excess, are generally manifested indirectly, in the production of diseases of various kinds. The privation is rarely ever absolute; the supply of food is inadequate to supply the wants of the organization, which requires daily animal or vegetable matter containing not less than nine ounces of carbon.

“All the sudden deaths are cases in which inquests were held. They would have been more numerous in the abstract, and so would the violent deaths, had it not been for some difficulties attending the registration of inquests. What is the nature of the sudden deaths, of which, notwithstanding, it appears in the abstract that .184 males and .118 females per 1,000 die annually? It will be observed that the proportion of sudden deaths is fifty-six per cent. higher in males than in females; the mortality in apoplexy is nineteen per cent.; in hæmorrhage seventy-eight per cent. higher in males than in females. It is probable, therefore, that sudden death is frequently the effect of hæmorrhage. It is certain that sudden death sometimes happens, without any appreciable change in the organization, at least any change which a rude, cursory, *post mortem* examination can detect; and it is not improbable that a certain number of cases of poisoning escape undetected by the coroners and the juries, who can be expected to know little of the symptoms either of poisons or disease, and are very rarely assisted, as in other countries, in their decision by the information which a careful examination of the body and an analysis of the contents of the stomach would furnish. The result of this negligence is, that little is known positively of the causes of sudden death; and the facility of procuring all the more intense poisons, as well as the prospect that the effects of poisoning *may be* confounded with natural causes, offers a strong temptation to the commission of that dreadful crime. Coroners' inquests are also held upon all prisoners who die in gaols, and the ordinary verdict is “Natural Death;” whence it would appear that the inquest in gaols is at present very much a matter of form, although it was no doubt instituted to ascertain the real cause of death, whether it were a common disease, or gaol fever, or dysentery, or violence. The causes of death registered as the result of a solemn, juridical investigation, are the most unintelligible in the register; as it is impossible to attach a specific idea to ‘Natural Death,’ to ‘Visitation of God,’ and to several other phrases in use in coroners' courts.

“As the age is precisely expressed in the registry, ‘Old Age’ is an unnecessary, and often an inaccurate term. But the diseases of the aged are exceedingly fatal, and the symptoms in their feeble functions are often masked, so that a pneumonia will run through

its course without attracting attention. The term ‘Old Age’ must therefore be preserved, until some considerable progress is made in the diagnosis of the diseases of old people. 5,674 males and 7,017 females were registered under old age; and I am not prepared to assert that in some instances vitality did not recede before tranquil insensible decay.

“4,845 violent deaths, 3,605 of males, 1,240 of females, were registered; and the annual mortality of males and females under this head was 1·048, and ·348 per 1,000, the males having suffered three times as much as females. The excess of males was 2,365, and it more than counterbalanced the mortality of childbirth. If all the violent deaths had been entered in the abstract, the mortality of males under this head would have probably equalled the mortality from typhus. This deserves attention. The individuals carried off by violence, typhus, consumption, and childbirth are in the meridian of life; and in a political sense their lives are of the highest value. Drowning in rivers and in the sea, burning, injuries in manufactories, explosions in mines, are frequent causes of violent deaths. Suicides are included under this head; the ages and sex of forty-four were distinguished.

“Ages of forty-four Suicides.”

Age. . . .	10-20.	20-30.	30-40.	40-50.	50-60.	60-70.	70-80.	All ages.
Males, . .	1	5	..	3	4	1	..	14
Females, .	5	7	2	3	8	3	2	30
Both Sexes,	6	12	2	6	12	4	2	44

“The notes to this section contain some curious causes of death. Six children, four males and two females, died in consequence of drinking boiling water from the spout of a tea-kettle, an accident which happens too often, and from which children would be saved, as well as from one still more frequent—burning—by a good fire-guard.

“Diseases of Towns and of the open Country.”

“Different classes of the population experience very different rates of mortality, and suffer different kinds of diseases. The principal causes of these differences, besides the sex, age, and hereditary organization, must be sought in three sources—exercise in the ordinary occupations of life—the adequate or inadequate supply of warmth and of food—and the different degrees of exposure to poisonous effluvia and to destructive agencies. The subsequent tables will exhibit the influence of the contaminated atmosphere of cities.

"The area of England and Wales is 57,805 square miles; and as the land population in the period under investigation was about 15,330,000, the number of inhabitants to a square mile was 265. The population is very unequally distributed; being thinly scattered over the agricultural districts, and accumulated at different intervals in villages, towns, and cities, where, as for instance in the metropolis, in the Unions of St. Giles and St. George, Bloomsbury, the Strand, the City of London, East and West London, Holborn, St. George in the East, and St. Mary, Whitechapel, the number of inhabitants to a square mile is 123,904. The population increased very slightly in these districts in the interval between the censuses of 1821 and of 1831; whence it may be inferred that the ground is nearly all occupied. In the East and West London Unions, however, the population is still more dense; the number of inhabitants to a square mile is 186,046. The greatest density attained in the heart of English cities is therefore nearly 243,000 inhabitants to a geographical square mile.

"The following table exhibits two sets of observations—the deaths in the metropolitan division with a population of 1,790,451, lodged upon an area of seventy square miles, and the deaths and fatal diseases in Devonshire, Dorsetshire, Wiltshire, Cornwall, and Somersetshire, where nearly the same number of inhabitants (1,723,770) was distributed over an area of 7,933 square miles. The five counties form the south-western division of England, and are bounded by the sea and a line running from Studland Bay to the Avon. (See TABLE C.)

"The population, 1st October, 1837, has been calculated from that ascribed to the respective counties in the population returns 1821 and 1831; it does not exactly coincide with the population of the Unions. The difference and any correction for omissions, either in the enumeration or the registration, may be neglected in the present comparison, as the total number of deaths in the one division was 15,220, in the other 24,959, giving an excess of 64 per cent. in the metropolis, while, in the census of 1831, the population enumerated in the metropolis amounted to 1,594,890, and in the five counties to 1,599,024. As, however, the population has increased at the rate of 1·8 per cent. annually in the metropolis, and at the rate of 1·2 per cent. annually in the five counties, it is necessary to calculate the population of the two divisions on 1st October, 1887, and this can easily be performed with sufficient accuracy. The following was the enumerated population of the five counties:—

	Census 1801.	Census 1811.	Census 1821.	Census 1831.
"Population	1,105,446	1,221,676	1,418,457	1,600,526*

* "Mr. Rickman gives two statements of the population of Cornwall. In vol. i. p. 48 of the Preface to the Population Returns of 1821, it is stated at 302,440; in vol. i. p. 80 of the enumeration, the population of Cornwall is stated at 300,938. The latter has been taken in the numbers at the head of the table, as the number of males and females is there distinguished."

“The annual rate of increase, 1801-31, was 1·24 per cent.; and if it be assumed that the rate remained nearly uniform up to 1st October, 1837, the population must then have been 1,730,606; the annual rate of increase, 1821-31, was 1·215, and this would make the population, 1st October, 1837, amount to 1,727,768, differing only 2,838, or 1-610th from the former number. The population increased 10 per cent. from 1801 to 1811, and 15 per cent. from 1811 to 1821; the former rate would make the population 1-68th below 1,730,606, and the latter would make it 1-60th above that number, an inconsiderable proportion, which may be considered the limit of error in the calculation. I have entered into these details, in one instance, to shew the degree of approximation which can be made by calculation to the actual population. The error can scarcely exceed 1-60th, or $1\frac{1}{2}$ per cent., while the deaths in the cities, in the two series of observations, (Tables C and D,) were from 50 to 60 per cent. more numerous than the deaths in the counties. It may be imagined that the difference in the half-year was accidental; but the proportion of deaths in the half-year ending 30th June, 1838, was not very different, and this, when taken in conjunction with the facts in Table D, is perfectly conclusive.

	Metropolis.	Five Counties.
“Deaths 1st July to 31st December, 1837, .	24,959 .	15,210
„ 1st January to 30th June, 1838, .	28,638 .	18,864
	<hr/> 53,597	<hr/> 34,074

“In comparing the deaths from different diseases in the metropolis with those in the counties, it will be recollected that the counties include Exeter, Plymouth, Portsmouth, and Portsea, Southampton, Bath, and a great number of towns; that small-pox was epidemic in Bath and Exeter; dysentery in Taunton; and that the health of this half-year in the entire extent of country was by no means favourable.

“The next table was framed upon the same principle; the fatal diseases of twenty-four superintendant registrars’ districts, with a population of 1,762,710, embracing several large cities and densely populated districts, is contrasted with the fatal diseases of Essex, Gloucester, Hereford, Norfolk, Suffolk, Sussex, and Westmoreland, comprising, after the subtraction of Norwich, Bristol, and Clifton, 1,776,980 souls. These counties were relatively healthy, and the contrast is striking. (See TABLE D.)

The fatality of every class of diseases, and of almost every disease, is augmented in the concentrated city population, but in very different degrees. This will be more evident in a tabular form, in which the facts of the two sets of observations are consolidated.

TABLE E.

Deaths by Twelve Classes of Fatal Diseases in City and in County Districts.

	CITIES.	COUNTIES.
Population	3,553,161	3,500,750
Epidemic, endemic, and contagious diseases	12,766	6,045
<i>Sporadic Diseases.</i>		
Of the nervous system	7,705	3,607
„ respiratory organs	12,619	7,847
„ organs of circulation	590	309
„ digestive organs	3,476	1,832
„ urinary organs	219	161
„ organs of generation	460	265
„ organs of locomotion	262	154
„ integumentary system	62	55
Of uncertain seat	4,396	3,730
Age	2,924	3,102
Violent deaths	1,370	929
Not specified	1,104	1,657
Total	47,953	29,693

“The concentration of the population in cities doubles the deaths from the two first classes of disease; the ratio of deaths having been as 1 to 2.11, and 1 to 2.13; and upon reference to the individual diseases in Tables C, D, it will be observed that the augmentation in the latter class occurs principally in convulsions and hydrocephalus:—Deaths by convulsions, counties 1,347, cities 3723, ratio 1 : 2.76; by hydrocephalus, counties 559, cities 1,540, ratio 1 : 2.75. It has already been intimated, that convulsion is a frequent intercurrent symptom in diarrhœa and diseases of the epidemic class in infants; it may exist, however, as an independent affection, and in that case has clearly, as well as hydrocephalus, with which it is allied, an epidemic character. A similar remark will apply to pneumonia and bronchitis, of which 1,209 cases were registered in the counties, 2,865 in the cities; ratio 1 : 2.37. The pulmonary inflammation was, in many cases, developed in the course of measles, influenza, and other diseases of the first class. The three following diseases, which principally affect adults, between the ages of 15 and 65, show that unhealthy places augment the fatality of diseases in different degrees.

	Counties.	Cities.	Increase per cent. in Cities.
“Deaths by Consumption .	5,857	8,125	. . 39
„ child-birth . .	217	372	. . 71
„ typhus . . .	1,564	3,456	. . 221

“This gives the classification a peculiar property. Wherever

the absolute mortality is low, the number of deaths in the epidemic class is less than the number in the pulmonary class; and, on the contrary, wherever the deaths in the first class exceed or equal those in the third, it may be affirmed that the absolute mortality is high.

“ The occupations in cities are not more laborious than agriculture, and the great mass of the town population have constant exercise and employment: their wages are higher, their dwellings as good, their clothing as warm, and their food certainly as substantial as that of the agricultural labourer. The poor law inquiry, and successive parliamentary committees, have shown that the families of agricultural labourers subsist upon a minimum of animal food, and an inadequate supply of bread and potatoes. The source of the higher mortality in cities is, therefore, in the insalubrity of the atmosphere. Every human being expires about 666 cubic feet of gas daily, which, if collected in a receiver, would destroy other animals; and is constantly producing, in a variety of ways, the decomposition of animal and vegetable matter, yielding poisonous emanations in houses, workshops, dirty streets, and bad sewers. The smoke of fires and the products of combustion are also poisonous. All gases and effluvia, like odours, are diffusible; they have a certain force of diffusion, which Professor Graham has expressed numerically; and all the emanations from human habitations in the open country mingle, almost as soon as they escape, in the currents of the atmosphere. But locate, instead of one individual to a square mile of land (the supposed density of population in the uncultivated forests of America and the steppes of Asia,) 200,000 individuals upon a square mile, as soldiers in a camp, and the poison will be concentrated 200,000 fold; intersect the space in every direction by 10,000 high walls, which overhang the narrow streets, shut out the sunlight, and intercept the movements of the atmosphere; let the rejected vegetables, the offal of slaughtered animals, the filth produced in every way decay in the houses and courts, or stagnate in the wet streets; bury the dead in the midst of the living; and the atmosphere will be an active poison, which will destroy, as it did in London formerly, and as it does in Constantinople now, 5-7 per cent. of the inhabitants annually, and generate, when the temperature is high, recurring plagues, in which a fourth part of the entire population will perish. But the health will be little more impaired by residence upon 1 than upon 100 square miles, if means can be devised for supplying the 200,000 individuals with 200,000,000 cubic feet of pure air daily, and for removing the principal sources of poisonous exhalations. The latter object is partly accomplished by paved, even streets, by the scavenger, by an abundant supply of water, by large well-constructed, trapped sewers, and by domestic habits of cleanliness; but it is difficult to perceive how volatile impurities can be removed, and how a stream of uncontaminated air can be supplied where the sun cannot heat the earth and air, where there are no open squares, or the streets are narrow, or the houses are only separated by courts, or built in *cul de sac*.

“ It will be found, *cæteris paribus*, that the mortality increases as the density of the population increases ; and where the density and the affluence are the same, that the rate of mortality depends upon the efficiency of the ventilation, and of the means which are employed for the removal of impurities. The next step in the argument is to establish these two facts ; which will be done by showing that in thirty-two districts of one large city the mortality increases with the intensity, and falls with the diminution of the causes, to which the excessive mortality has just been ascribed. (See TABLE F.)

“ The next table will exhibit the principal classes of diseases to which the mortality in the different districts was due. The population (1837) was deduced from the censuses of 1821 and 1831 ; no correction was made, either for the population unenumerated, the deaths in the hospitals, or any deficiency in the registers.

“ Not to attach undue weight to the separate observations, the mean of the first, second, and third divisions has been taken.

TABLE H:

Exhibiting the Mean Mortality, in Three Groups, of the Thirty-two preceding Districts.

Districts.	Square Yards to one Person.	Annual Rate of Mortality per 100.	Annual Rate of Mortality per Cent. by Diseases of						
			The Epidemic Class.	<i>Typhus</i> .	The Nervous System.	The Respiratory System.	<i>Phthisis</i> .	The Digestive Organs.	Other Classes.
1 to 10, (mean)	57	3.321	.991	.324	.543	.822	.478	.208	.758
11 to 20, „	78	2.839	.701	.205	.467	.768	.451	.197	.706
21 to 30, „	217	2.163	.485	.107	.369	.588	.354	.155	.567

“ The mortality then increases with the density, yet the densest districts are not invariably the most unhealthy.

Unions or Districts.	Area in square Yards to one Person.	Annual Rate of Mortality per cent.
St. James, City of London, Strand (mean)	24	2.1
Shoreditch Bethnal Green, Bermondsey (mean)	60	3.1

“ The necessary deduction from the double series of facts, then is, that the mortality has a tendency to increase as the density of the population increases, but that the unhealthful tendency can be counteracted by artificial agencies. In other terms, the mortality of cities in England and Wales is high, but it may be immeasurably reduced.— A good, general system of sewers ; the intersection of the dense, crowded districts of the metropolis by a few spacious streets ; and a park in the east end of London, would probably diminish the annual deaths by several thousands, prevent many years of sickness, and add

several years to the lives of the entire population. Similar improvements would have the same effects in the other cities of the empire. The poorer classes would be benefited by these measures, and the poor-rates would be reduced; but all classes of the community are directly interested in their adoption, for the epidemics, whether influenza, typhus, or cholera, small-pox, scarlatina, or measles, which arise in the east end of the town, do not stay there; they travel to the west end, and prove fatal in wide streets and squares. The registers show this; they trace diseases from unhealthy to healthy quarters, and follow them from the centres of cities to the surrounding villages and remote dwellings.

“ In the unhealthy districts of the metropolis, as in tables C, D, and E, the mortality from the epidemic class of diseases is higher than the mortality from diseases of the respiratory system; in the healthy districts, on the contrary, the mortality by the former is lower than the mortality by the latter class of diseases.

Annual Rate of Mortality per cent.

Districts.	From the Epidemic Class of Diseases.	From Diseases of the Respiratory Organs.	Absolute Mortality from all Causes.
1—10 (unhealthy)	·99	·82	3·32
11—20	·70	·77	2·84
21—30 (unhealthy)	·48	·59	2·16

“ The reason of this is obvious: diseases of the epidemic class increase more rapidly than diseases of the respiratory system in unhealthy localities. The relative increase will be understood at a glance, when the mortality in the first ten districts is taken as unity.

	Districts 1—10.	Districts 11—20.	Districts 21—30.
Epidemic diseases	1·00	1·45	2·04
Diseases of the res- piratory organs	1·00	1·31	1·40
Typhus	1·00	1·92	3·03
Phthisis	1·00	12·7	1·35

“ The fatality of typhus increases 303 per cent. in the districts 21—30; phthisis (consumption) 35 per cent.; and it may be laid down as a general principle that wherever the proportion of deaths from phthisis, compared with the total deaths, is high, the absolute mortality is low, and that the absolute mortality from phthisis itself is low. Attention to this fact will obviate several practical errors, such, for instance, as sending consumptive patients to the West Indies. The deaths out of the living express the real tendency to phthisis.

	Districts 1—10.	Districts 11—20.	Districts 21—30.
Proportion of deaths by phthisis in 100 <i>deaths</i> .	1·44	15·8	16·4
Annual deaths by phthi- sis out of 100 <i>living</i> .	·478	·451	·354

“ These observations will facilitate the interpretation of Table N ; they will, at least, prevent the facts which it exhibits from being misunderstood. But before proceeding to the rest of the Tables, I subjoin another illustration of the principles above stated in a comparative view of the mortality of females in five cities. The population, October 1, 1837, was deduced from the annual rate of increase 1821—31, and the proportion of females was assumed to be the same as in 1831.

TABLE I.

Districts.	Population.				Total Deaths.	Area in Square Miles.
	1821.	1831.				
		Males.	Females.	Both Sexes.		
Leeds	94,638	66,555	69,026	135,581	1,582	64·8
Birmingham	85,416	54,593	56,321	110,914	1,459	4·2
Manchester and Salford	176,629	113,842	123,093	236,935	3,703	59·5
Liverpool and West Derby }	151,331	101,015	117,218	218,233	4,913	59·5

TABLE K :

Of the relative Mortality of Females in Leeds, Birmingham, Manchester, London, Liverpool ; and in England and Wales.

Annual Deaths to 100 (000) living.

	Leeds.	Birmingham.	Manchester, &c.	London, &c.	Liverpool, &c.	England and Wales.
Epidemic, &c. diseases	·369	·418	·565	·697	1·116	·459
<i>Typhus</i>	·115	·126	·180	·196	·339	·129
of the nervous system . .	·309	·250	·414	·400	·544	·284
of the respiratory organs	·515	·623	·722	·657	·860	·551
<i>Phthisis</i>	·401	·494	·510	·402	·670	·415
of the organs of circulation	·009	·054	·013	·033	·026	·019
of the digestive organs . .	·127	·214	·209	·177	·228	·133
of the urinary organs . .	·007	..	·003	·004	·001	·004
of the organs of generation	·058	·051	·058	·052	·058	·042
of the joints	·016	·012	·019	·014	·007	·012
of the integumentary system	·004	·002	·010	·003
of uncertain seat	·166	·229	·161	·270	·214	·232
Old age	·138	·156	·128	·218	·163	·197
Violence	·035	·045	·042	·032	·046	·035
Unspecified diseases	·055	·048	·040	·034	·016	vid.p.72
All causes	1·804	2·101	2·378	2·590	3·289	1·972
Inhabitants to a square mile . .	2,624	31,487	4,799	25,578	4,617	265

“ In all these divisions, as well as in the metropolis, the great mass of the population is concentrated upon a few square miles. Thus the population of Manchester, 168,911, was located upon 1,480 acres (2·31 square miles) : while the population of Blackley and the rest of the district, amounting, October 1, 1837, to 29,453, was distributed over 12,410 acres. (19·39 square miles.) And in Liverpool, 203,327 inhabitants resided upon 1,560 acres, (2·43 square miles); in West Derby 72,571 upon 36,500 square acres. The rate of mortality and the diseases vary with the density of the population in Liverpool and Manchester.

TABLE L.

LIVERPOOL (Division 3.)					
	Area in Acres.	Population October 1, 1837.	Deaths July 1 to Dec. 31, 1837.	Inhabitants to Square Mile.	Annual Rate of Mortality per cent.
Liverpool . .	1,560	203,327	3,996	83,415	3·931
West Derby . .	36,500	72,571	917	3,107	2·527
Difference	1,404
MANCHESTER.					
Manchester . .	1,480	168,911	2,718	73,121	3·218
Blackley, &c. .	12,410	29,453	258	1,514	1·752
Difference	1·466

“ The Tables A 1, A 2, &c., present the deaths from different diseases in 25 great divisions into which you have distributed England and Wales ; and Table N has been deduced from the Tables A, A 1, &c. Both the tabular views are highly interesting, and deserve a careful examination, in connexion with the facts which have been taken from the tables of ages, and placed at the head of the Tables A, A 1, &c.

“ I have not been able to examine the influence of age, seasons of the year, and other circumstances, upon the fatality of diseases ; and there are other omissions which you, Sir, will not fail to notice, and I venture to hope, excuse, in the first essay. The publication of the deaths from each disease in the different divisions will enable statistical writers to supply the many deficiencies of this letter ; and, with adequate assistance, ensuing abstracts may be rendered more complete and satisfactory.

“ I have the honour to remain, Sir,

“ Your very obedient and humble Servant,

“ To the Registrar-General.”

“ WILLIAM FARR.”

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.—(CONTINUED.)

April 4th.

Dr. LABATT in the Chair.

Mr. Bovell read a paper on uterine hæmorrhage occurring after the sixth month of pregnancy, but before the birth of the child. Having noticed Rigby's judicious division of the subject into accidental and unavoidable, he explained the manner in which the hæmorrhage is produced in both cases, and detailed the causes liable to induce the former. He then pointed out the treatment necessary in each case. He dwelt especially on the use of cold, and the circumstances under which it becomes inadmissible; the benefits to be derived from the tampon, and the cases suited to its employment; and the utility and caution to be used in administering the ergot of rye for the purpose of arresting hæmorrhage.

An interesting discussion followed the reading of this paper, which turned principally on the use of the plug in placenta presentations. Dr. Kennedy alluded to a manner in which he had used the plug where the placenta partially presented. He stated that he had derived much benefit from it in restraining the hæmorrhage during the time we are obliged to await the dilatation of the os tincæ, in those cases in which the edge of the placenta is found to overlap it. He explained the dependence of the hæmorrhage, in these cases, upon the separation of the placenta from the neck of the uterus, and the consequent exposure of the vessels at this stage of the labour, and observed that in order to correct this state, he had been in the habit of carefully introducing a piece of sponge *between the head of the child and the placenta*, thus pressing the latter against the neck of the uterus and closing the mouths of the vessels. This he has allowed to remain, and act as it were the part of a compress, until the head descended into the pelvis, the head, in its progress, adding to the compression and more completely restraining the discharge of blood. He stated that he had by this means, in several cases, completely restrained the hæmorrhage, and the patients had been delivered of living children without the necessity of turning or other interference. He cautioned the Society, in case of their having recourse to this method, of the care necessary to take in getting up the sponge, and bringing it against the amnial surface of the placenta, as, if pressed up between the uterus and placenta, it must separate them still further, and consequently increase the hæmorrhage. Dr. Kennedy further observed, that it was not his wish, in mentioning this plan of practice, to lessen the confidence justly placed in turning in placenta presentations, and stated the necessity which exists, after using the plug, for watching the patient closely, and if the circumstances of mother or child require it, having recourse to turning, or the application of the forceps. He then mentioned two cases which had occurred under his observation, in which the placenta separated

from the uterus, and descended before the head of the child. In the one there was a total absence from hæmorrhage, in the other a comparatively trifling loss.

Dr. Sargent mentioned, but did not from his own experience advocate the use of an instrument devised by Dr. Torbeck of Newcastle, for the repression of hæmorrhage. It consisted of a gum elastic hollow ball with a tube and stop-cock, by means of which it could be distended with fluid to any dimensions, and consequently the degree of pressure graduated according to the urgency of the case. One of the advantages said to be derivable from it was, that an examination could be made without its removal.

Dr. Dwyer thought it an ingenious instrument, but not one which could be used with advantage in such cases. The plug introduced between the head and the placenta he has seen in the hands of Doctor Kennedy afford satisfactory results, but if not used with the greatest care, its employment may place the woman in greater jeopardy than before.

Dr. Kennedy thought the instrument proposed by Dr. Torbeck might be applicable when sloughing of the vagina causes hæmorrhage by opening vessels, as in a case then in hospital, which had been sent in with an extensive rent into the bladder, consequent on turning, and in which its edges had taken on the process of sloughing.

On the conclusion of this discussion, Doctor Kennedy exhibited the preparations and reported two cases which lately occurred in the hospital, wherein the os uteri had separated spontaneously during labour.

The particulars of the cases are as follows :—Catherine Kelly was delivered in hospital of her sixth child on the 7th of March last, after a labour of seven hours. Ten hours after delivery* attention was directed to a fleshy substance, protruding from the vulva, which made its appearance externally after the expulsion of the placenta. It was found connected with the os uteri anteriorly and to the right side, and was evidently two-thirds of the labia of the os. The remainder he separated by torsion, and the whole was found completely to correspond to the neck of the uterus; no hæmorrhage or other constitutional symptoms followed. The other case (that of Curtis, pregnant for the first time) was one of tedious labour, arising from a congested and undilatable state of the os uteri, with a pelvis of rather undersized dimensions. On the 1st April, at 10 A. M., os dilated to size of half a crown, and beginning to be œdematous; pains frequent; waters discharged; tartar emetic given with little effect. On the 2nd, 10 A. M., os two-thirds dilated, very much congested, of a deep

* In the report of the Proceedings of the Pathological Society, before which these preparations were also exhibited, a misprint occurs with respect to this case, in stating, that the os uteri was removed two or three hours before delivery. Dublin Journal for July, page 503.

purple colour; pains not frequent; anterior lip scarified. At 9 P. M. os somewhat more dilated posteriorly; head had descended a little. An attempt was made to support with the fingers the anterior lip during the pains; the posterior part spontaneously separated, and appeared without the vulva. The remainder Dr. Kennedy removed. It was ultimately found necessary to deliver this woman with the crotchet. She had a tedious convalescence, owing to a tendency to the occurrence of peritonitis, and the formation of adhesions in the vagina. On separating the newly formed adhesions on one occasion, a quantity of foetid pus flowed from the cavity of the uterus. She was ultimately discharged well. Only two similar cases are on record; one reported by Steidale, in Wasserberg's Dis. F. 1. Com. Lip. xxi. p. 518; and the other by Mr. P. N. Scott, in the Lond. M. Repos. No. 97, Jan. 1, 1822.

May 2nd.

Dr. COLLINS in the Chair.

Dr. Doherty read some observations in reply to Mr. H. Carmichael's views of the mode of growth and contraction of the uterus, which appeared in the last Number of this Journal, and Dr. Kennedy followed with his paper on occlusion of the vagina, which will be found amongst the original communications in the present Number.

June 6th.

Dr. LABATT in the Chair.

Dr. Sargent read a paper on some practical points connected with variola. Having made some preliminary observations on the importance of the subject, the probability of the ancients being familiar with the disease, and the benefits vaccination has conferred on the community, he proceeded as follows:

Upon the presumed identity of cow-pock and small-pox, I will now make a few observations; a subject which attracted much of the attention of Jenner, and his opinion on it was one of his most cherished doctrines; as, if it were true, it would in a great measure, if not altogether, decide the most practical question at issue. To prevent misapprehension, it will be well to state Jenner's opinion in his own words; I quote from one of his published Journals.

"The origin of small-pox is the same as that of cow-pock, and as the latter was probably coeval with the brute creation, the former was only a variety springing from it. It will be inquired, in what way can the action of cow-pock, in preventing subsequent small-pox, be reconcileable with the established laws of the animal economy; my reply is, not that they were *bona fide* dissimilar, but on the contrary identical."

Were the above brief extract minutely examined, it would present a mass of as unmeaning assertion, and as false inference, and as absurd pathology, as could be well crowded into the same

number of words. *The origin of the two diseases being the same, one is therefore a variety springing from the other*; even were the antecedent proposition true, for which there is not a shadow of proof, the consequent by no means necessarily follows: so much for logic; nor is the pathology more legitimate. *In what way, asks Jenner, can the preventive power of cow-pock be reconciled with the known laws of the animal economy?* Does it not strike the veriest tyro in pathology, that we know absolutely nothing of the laws of the animal economy, in health or in disease, but from the observed phenomena, and this preventive power being once satisfactorily proved by observation, it immediately becomes *itself* as much a law of the animal economy as any other principle dignified with the title, nor is there any other principle to which it is directly opposed, and for which we must search for a reconciliation. In fact, it were as reasonable to ask how the action of sulphate of quinine, in preventing the recurrence of intermittents, could be reconciled with the known laws of the animal economy; can we not here trace a subtle recognition of the even more modern doctrine homœopathy, the "*similia similibus curantur*," of Hanheman.

But it is the old sophism of inventing or assuming a premise quite as debateable, if not identical with the conclusion, we wish to arrive at; but as the proposition is merely implied, not actually expressed, the fallacy will strike any one but the happy and contented theorist. In the present instance the reasoning, if it may be called such, appears to be as follows.

No dissimilar disease can prevent the invasion of another; but cow-pock prevents the accession of small-pox; therefore those diseases are not dissimilar, but identical. Probably the "*well known law of the animal economy*," is the first proposition which there is reason to fear was assumed by Jenner, in this instance, to prove what he deemed necessary to the full establishment of his glorious discovery; but the same Jenner erected this, the most triumphant and beneficent monument of medical science, on a surer basis than an unproved theory, even on the basis of patiently diversified experiment, and the result of almost unlimited success; and it is on these solid foundations rests his own imperishable renown.

Unacquainted as we are with the essence of the vital principle, we are consequently unable to state what is the final and essential cause of diseased manifestations. If it be inquired then what constitutes a distinct disease, we may answer, a certain combination of functional derangements, or similar organic changes, or what is more usually the case, a combination of these two phenomena considered together, as associated and mutually dependant. Of these latter or mixed cases, the eruptive fevers form a striking group of examples, in each of which we find a certain train of functional phenomena, ushering in a distinct and characteristic inflammatory affection of the skin. With this view of the distinct characters of disease, can we trace any identity between the two affections, they being different not only in the peculiarities of the febrile invasion, but also most dissimilar in their very characteristic eruptions,

this latter character placing them, according to the accurate pathologists Willan, and Bateman, in altogether different orders of skin diseases.

But it must be confessed, that we are sometimes compelled to abandon this view of the identity of disease,—such are the Protean forms of some maladies,—and are forced to designate by the same name, two groups of symptoms which at first sight appear altogether different: take for example, a patient seized with the most alarming symptoms of irritative fever; treat as you will, before many hours have elapsed, the brain becomes oppressed, a dark lurid discoloration, without tumefaction, appears, in the fauces, low muttering delirium sets in, all the symptoms denominated typhoid ensue, and the patient sinks perhaps on the third day. Another patient is found to be covered in the morning with a diffuse exanthematous eruption, which increases until he is as red as a boiled lobster from head to foot; the constitutional symptoms were imperceptible, we discover neither quickened circulation, impaired appetite, or diminished innervation; in a few days the cuticle separates in detached portions, and the patient is restored to perfect health. However diverse these two cases may appear in their progress and termination, the physician does not hesitate to designate them by the same name—Scarlatina: but why does he do so? not without grave and irresistible evidence of their identity; they have been caught from the same contagion, they mutually propagate each other, and above all, these extreme cases are united by others exhibiting every gradation of resemblance to them, making it impossible to draw any line of demarcation between the two. Now I would ask, does any similiarity of this kind exist between variola and cow-pock? I believe there does not. Are they ever caught from the same contagion? Never. Do they ever propagate each other? Never. Millions of vaccinations have taken place in Europe during the last forty years, and is there on record a single case of variola produced? I believe there is not. Jenner insisted much on the results of inoculation, diminishing the number of pustules, but pustules they still remained. Indeed Jenner did not appear over refined or particular in his nomenclature of eruptions, and the vesicle of cow-pock is converted into “*the single benign, pearl-like pustule.*” Some accounts have been recently published from India, detailing a severe pustular disease from vaccination, and on the evidence of Mr. Wood of Gowalpara, life was endangered, so that he even thought inoculation itself might be deemed preferable, these reports have been greedily seized on by Dr. Baron, and other advocates of the identity of the two diseases, but the evidence is too partial to be decisive. It is very well known that black cattle are subject to different forms of eruptive disease, not only diverse in degree, but in kind, and if the same facts were adduced in proof of the dangers of vaccination, this is the argument that would be at once urged. But in the midst of these uncertainties, can any argument from direct experiment be adduced to set the question at rest; or at least to afford a very strong presumption on their being non-identical in nature. Not to enumerate all the experiments

which have been instituted for this purpose, and their results, I shall merely advert to two, the first which was frequently tested at the London Small-pox Institution, the other occurring fortuitously in my own practice, appeared to me at the time of its occurrence decisive on the question. In the first, it was proved that if vaccine and variolous virus be inserted into the arm so nearly, that on the formation of the pustule and vesicle they coalesce, they will notwithstanding retain their distinctive character, one side producing genuine small-pox, the other genuine cow-pock. This experiment I would not perform myself, nor would I ever be a party to variolous inoculation in any mode; I never witnessed but one inoculation, the result was confluent small-pox which proved rapidly fatal; the operation was performed by a gentleman on his own child; he was not a medical man. The other fact is the following; during the spring and early part of the summer in 1837, small-pox was prevalent, frequently occurring, and in an aggravated form, after vaccination. On June 3rd, I vaccinated master J. C. with lymph taken from his sister's arm; another sister was vaccinated from the same source, and passed through the different stages most satisfactorily; that evening the boy was taken ill; he laboured under nausea, quick pulse, heat of skin, epigastric pain, hoarseness, with slight tumefaction of the fauces, &c. &c.

It is unnecessary to detail the process and treatment of a severe case of small-pox which ensued, he was in fact labouring under the latent period of the disease, when the vaccine lymph was inserted into his arm; during the eruptive and maturing periods, I in vain looked for any effects from the vaccination, but when the maturation was complete a vesicle rapidly formed, and on the twelfth day of the disease it was in full maturity, surrounded by the incrustations of the dying small-pox. One other case of similar character I saw this year in dispensary practice; the vaccine vesicle appeared with its distinctive characteristics amidst the crusts of confluent small-pox, which proved fatal. I had not an opportunity of observing the particulars, as in the former case, which occurring in private practice, and watched from day to day, prevented any mistake or misapprehension. Another case, strictly analogous, has been related to me by Dr. Kennedy, the learned President of the College of Physicians, in which the cow-pock was suspended by the eruption of small-pox, and afterwards ran its regular course, when maturation had been completed. The case struck him particularly at the time, he took an accurate note of the particulars, and preserved some of the lymph from the vesicle, with the intention of testing its distinctive character by vaccinating with it. These cases prove, if any can, the non-identity of cow-pock and small-pox. If it be argued, notwithstanding, that although those *forms* are different, yet that they are *essentially* alike, or in other words that they are *generically* identical, but *specifically* different, this will be only falling back on the endless and metaphysical subtleties connected with the arbitrary distinctions between genera and species, which, when too far pursued, tend only to confound and embarrass the progress of the natural sciences, even

those in which the distinctive characters are more palpable and permanent than in pathology; for instance, botany and zoology.

One other subject connected with the disease, and I have done. We know that scarlatina frequently exists, in its stage of febrile invasion, without presenting any affection of the skin, giving rise to a contagion producing the ordinary eruptive form of the disease. The question I would propose is, *does any thing analogous occur in variola?* In the partial epidemic of 1837, I met with some cases of fever, which presenting all the peculiarities of that ushering in variola, I was led to expect the eruption, but it did not appear; there existed the peculiar epigastric pain, deep roughness of the voice, and salivation, along with the ordinary phenomena of quickened circulation, heat of skin, &c., which ceased suddenly the third day, and these cases occurred in neighbourhoods where variola was prevalent. From these patients having been vaccinated, I was led to expect the disease in a modified form, *was I justified in concluding that it was the modified small-pox?* Shewing that one of its modifications is variolous fever without eruption, we may be assisted in our conclusions by considering what occurs in admitted cases of modified variola, that after very sharp attacks of fever, not more than a dozen pustules may arise, and even these present a very imperfect type, suppurating only at an elevated point of the hard papula which first appear; what limit can we ascribe to the diminution of these pustules? Are not all practitioners aware of severe attacks of fever, which go off suddenly on the appearance of a very few elevated papula, which never go beyond their first form? I have known the little daughter of a physician to be seized with intense symptoms of fever, a convulsion ushering in severe headach, (not an unusual occurrence in variola, occurring in children;) the little patient was treated most anxiously for hydrocephalus, but no treatment appeared to diminish the constitutional symptoms, which ceased suddenly the third day, on the appearance of a very few papulæ, I believe not more than three or four, which remained elevated and hard until they disappeared. May not this and similar cases be referred to those attacks of modified small-pox, which, according to a most experienced writer on the subject, are characterized merely by "*a few scattered pimples?*" and thus we are brought to the very threshold of the admission of a *variolous fever without eruption*. This I do not propose as a mere pathological curiosity of theoretical interest only, but if such affection really occur, there are numerous inquiries of a deeply practical nature intimately connected with it: but although the question was not the result of an abstract inquiry, but was forced on my attention by the cases I have alluded to, I would not presume to imagine the point to be decided by the few cases occurring in the experience of a single practitioner. I may here quote a passage from Sydenham, which appears to bear upon the question, "at the same time that small-pox first appeared," (says he, in one of his treatises on this disease,) "there arose a new kind of fever, not much unlike it, except in the eruption of the pustules and the symptoms thereupon depending." The latter clause is a little obscure, but I suppose we

are to understand by it, that the eruption was altogether absent. In his treatise on measles, he distinctly states that a morbillous fever without eruption prevailed during the epidemic occurrence of that disease, but it is not mere analogies which should decide the existence of the affection in question, however they may prepare our minds for the admission of it when fairly presented to them by a sufficiently extended clinical observation.

On the conclusion of this paper an interesting discussion ensued, of which the following is a summary :

Dr. Harty stated that a gentleman lately came to town with his children, several of whom had been inoculated ; and called upon him for the purpose of having his youngest submitted to the same process. Dr. Harty vaccinated it, and the disease ran its usual course. Knowing this gentleman's history, for he was the son of a physician in Cavan, heir to an estate, provided he arrived at full age, and his father, anxious concerning him, had him inoculated repeatedly, but without effect. Being aware of these circumstances Dr. Harty asked him, if he had ascertained whether he could himself take vaccination. He replied not. Dr. Harty then vaccinated him from his own child, and he gave every aid to the success of the operation ; not resting satisfied with the punctures made, he actually inserted into his own arm a needle and thread, which he had armed with virus from the same source. Notwithstanding which not the slightest effect was produced. The punctures dried up, and he shewed himself insusceptible. The child's arm became highly inflamed next day, and it was only afterwards this gentleman explained the cause. This case is very conclusive as to his insusceptibility of both. It is only a single fact, however.

With respect to the occurrence of variola after vaccination, Dr. Harty related the following facts : A merchant had four sons and four daughters, all of whom had been vaccinated by the late Dr. Law and Dr. Clarke, high authorities in their day. One daughter, when she arrived at eighteen years of age, was seized with smart eruptive fever, followed by the appearance of distinct variolous pustules ; the fever then abated, and she went through the regular course of small-pox. Afterwards her eldest sister had an acute eruptive fever, which required her to be bled twice, and she proved a most formidable case of confluent small-pox ; her sufferings were extreme, not a point of the body which was not covered with eruption. At the period of the secondary fever she had slight rigor, and then a rapid abatement ; at first she was strongly marked, but this after several months disappeared. With respect to the other children, every one had slight variolous fever ; but so slight, that it would at another time have been unnoticed. A very trifling eruption ensued, and in some Dr. Harty believes fever existed, without any eruption. The four junior children were soon after re-vaccinated, and not the slightest mark of the true vesicle followed.

Dr. Harty further remarked, that practitioners know that in measles there may be a distinct eruption without catarrhal fever, and that this affection gives no security against a renewal of the

disease. In one case he witnessed a return in less than three weeks afterwards; he might therefore fairly conclude that fever may exist without eruption. Sydenham speaks of small-pox fever without the eruption, and dysenteric fever without the dysentery; there therefore may be a distinct fever, independent of any eruption.

Mr. Speedy knew the children of a clergyman, all of whom had been vaccinated some years before; one of them was taken ill, and as the child had been exposed to the contagion of small-pox, he deemed it prudent to vaccinate his brothers and sisters, and to remove them immediately to the country. The first child went through small-pox; the others had cow-pock first, and on the twentieth day a modified fever, and then a few well marked pustules; this was their second vaccination, and it was perfect.

Dr. Labatt was certainly of opinion, that where both diseases are received into the system, the small-pox modifies the vaccine vesicle, if it occurs first, and *vice versa* the vaccine modifies the variola when it occurs.

Dr. Breen has re-vaccinated extensively, and would say he never succeeded in producing a perfect vesicle. The areola forms earlier, and the vesicle is premature. He also formerly, but not recently, inoculated subjects previously vaccinated with variolous matter; but never succeeded in producing any thing like an approximation to the variolous disease, either locally or constitutionally.

Mr. Parkinson in two families re-vaccinated several individuals, and they went through it perfectly.

Dr. Kennedy having alluded to Dr. Lohmeyer's Report of Re-vaccination in the Prussian Army, in which it was reported as regular in the proportion of nineteen out of forty-two cases, stated that his experience coincided more with that of Dr. Breen. He mentioned that he had re-vaccinated nine members of one family, from twenty-two years of age to nine months, and in no instance did it run its ordinary course, except one, in whom there was a good deal of question as to the efficacy of the first vaccination. The areola in such cases forms about the fifth or sixth day; it is hasty and imperfect, completing itself before the eighth or ninth day. In allusion to Dr. Sargent's remarks upon variolous fever without eruption, Dr. Kennedy stated, that he had lately attended with Mr. Atkinson a patient labouring under variola after vaccination. It ran its usual course, and no unfavourable symptom ensued: and he remembered that two young children in the house, one a year and a half old, and the other three years, were at the same time attacked with a suspicious febrile paroxysm, which terminated without eruption. This may have been a coincidence, but the subject is one of great interest. As to the efficacy of cow-pock as a preventive against variola, he instanced the case of a gentleman affected with the disease after vaccination, with whom his seven children were constantly in the house, and even in the very bed, and yet not one of them took it, as they had been previously vaccinated.

Dr. Labatt was of opinion, that the longer the period from the first vaccination, the more likely will a genuine vaccine vesicle be produced by a second; but the Doctor would not infer that these patients would have taken the small-pox, if exposed to its infection. He in a great degree agreed with Dr. Breen, but he had seen the true disease revived after the lapse of twelve or fourteen years.

Dr. Breen sometimes tried a third vaccination, with intervals of from two to eight or nine years; and the third, as well as he could recollect, ran very nearly the same course as the second.

Dr. Labatt within the last eight years re-vaccinated four sisters, who had been vaccinated in London when infants; their ages were then from fifteen to nineteen; three had the vaccine arm. The result of this operation was, in three of them to produce a perfect vesicle; in the other it failed, though she had not the vaccine arm.

Dr. Breen once tried, with permission of the parent, the effect of matter taken from the vesicle in a re-vaccinated patient, which approached somewhat to the regular vaccine vesicle, but it produced no appearance on the arm where it was inserted.

Dr. Horne was lately called to see the child of a poor man. It had fever and diffuse small-pox over its body, and none of the family had been vaccinated. He immediately inserted cow-pock matter into the arms of all the other members of the family, the perfect vesicle was produced, and though all slept in the same bed not one more became affected with variola.

Dr. Labatt stated, that twenty-six years ago a child came to him on the eighth day after vaccination. It was feverish, and supposed to labour under vaccine fever. Others were vaccinated from it by five practitioners, to the number of sixteen or eighteen children. The fever in the child first alluded to increased; on the ninth or tenth day, the face swelled, and on the thirteenth confluent small-pox declared itself. He immediately apprised the gentlemen who had used the matter so obtained, but every case took cow-pock genuine, with no variolous eruption, excepting in one child in whom it failed altogether. He agreed with Dr. Harty, that where the constitution shews an immunity to the one disease, it will also to the other. He could find four cases in his note book, where he failed with both.

Dr. Ryan was attending a gentleman, who had not been vaccinated, labouring under confluent small-pox. The disease proved fatal, and an examination of the body was made by him and two other gentlemen. Dr. Ryan (though he had been in infancy vaccinated by Dr. Labatt himself) had symptoms of fever the day after, in two days inflammation and furunculus of his arm. A young gentleman, who just arrived by the steam packet from Belfast, went up to see him, and looked at his arm. He was attacked six days after with rigor, succeeded by fever and confluent small-pox. He had been vaccinated at Paris, yet so severe was the disease, that for ten months the pits were not removed.

Mr. Gibson remembered, that when he was at the Richmond Hospital, a portion of a subject, which had died of small-pox, was apportioned to a fellow-student who had been vaccinated, and he soon after was affected with confluent small-pox.

Dr. O'Reilly, in confirmation of Dr. Labatt, stated the case of his own child, who, when vaccinated, was in the constant company of the child of the widow of an officer, who had come on a visit to his house. And though this child had confluent small-pox, the former did not take it. Dr. O'Reilly then inoculated his child from his companion, but the small-pox never came to perfection; he then re-vaccinated him, and the result was, that the vesicle went through its stages with great rapidity.

Dr. Sargent was of opinion, that variola cannot be taken from a dead body in any way. He heard of a man who inoculated from a corpse affected with the disease, but symptoms of putrescency alone followed, and no eruption of small-pox in any instance.

Lectures on Variola, by N. Chapman, M.D., Professor of the Theory and Practice of Physic in the University of Pennsylvania.—My attention is next to be directed to the consideration of eruptive fevers, commencing with variola, or small-pox. This is a disease of modern times; no account of it is to be met with in any of the writings of Greece or Rome, which have descended to us. Endeavours have been made by Willan and others to trace it to antiquity. But, elaborate and recondite as were their researches, they have not produced any satisfactory evidence of its existence, and the fact, as stated, is now sufficiently conceded. It has, on this point, been forcibly urged by Sydenham, Mead, and Freind, that since Hippocrates, and especially Celsus and Galen are silent in regard to it, the works of the two latter being a sort of a digest of the knowledge of their predecessors, we are entitled to this conclusion, and the more so, from the precision of their history of diseases, that no such had occurred, or with which they were acquainted.

The earliest notice of it is by the Arabian writers. An old manuscript in the library at Leyden, dated 572, declares, that, "in this year, small-pox and measles made their appearance in Arabia." It seems, however, that several years before it broke out, at the memorable siege of Mecca, where it raged with great violence in the Christian army, leading to its total discomfiture. This event happened, according to Gibbon, the historian, two months prior to the birth of Mahomet.

To Rhazes, a most distinguished man, who lived at the commencement of the tenth century, we are indebted for the first full and accurate description of small-pox; and he states, that it was brought out of Ethiopia into Arabia. To the writings of his predecessors long since lost, by whom it had been previously noticed, he, however, refers, and especially to those of Ahron, a physician of Alexandria, in Egypt. The latter resided in that city, in 641,

when attacked by the Arabians under Omar, the successor of Mahomet, and it is not improbable, the disease was conveyed to it by the invading army, and in this way he became conversant with it. Comparing dates, we shall find, that this was seventy-nine years after the siege of Mecca, on which occasion, so far as ascertained, small-pox, as I have said, sprung into being.

Nevertheless, by some it is supposed, that it originated in China, or the remoter India, or that, at least, it was known in these regions for centuries anterior to the period I have mentioned. But medical writings not existing, or to which access can be had, among these people, this opinion, not resting on authentic records, is a mere deduction from their mythology, religious institutions, some allusions in their civil history, their traditions, and other sources equally vague and inconclusive.

Considering, however, the intimate connexion of the Arabians with the East, they might have derived it from that quarter of the world. Be this as it may, we have the most satisfactory proof of its introduction and diffusion through Spain, Sicily, and the Levant by the Saracens, when they, in the eighth century, overran these countries. But though it had gained a partial admission into Europe, it did not generally, till the close of the twelfth, or the beginning of the thirteenth century, when the Crusaders were engaged in the Holy Wars.

Contracting the contagion in Palestine, these bold and enthusiastic adventurers introduced it, on their return into their native places. The intercourse of the nations of Europe with each other, becoming greatly extended by commerce, about this time, it spread rapidly throughout Christendom, and, for several hundred years, its ravages were terrible. Nor were these, subsequently, in our own hemisphere less extensive in proportion to the number of subjects. Conveyed to it by the successors of Columbus, the tale of misery and desolation it inflicted is painful to peruse. Twenty-five years after the discovery of this continent it occurred, and we are told, that it destroyed more than a moiety of the population of the provinces into which it penetrated. Three millions and a half are computed to have fallen victims to it, in a very short time, in the kingdom of Mexico alone.

Brought afterwards by emigrants from Europe to our immediate land, it swept off also several tribes of the aborigines, leaving scarcely a sufficiency of them to preserve their name. Gradually it reached other and obscurer regions, owing to the enterprises of discovery and exploration, or to the slower encroachments of civilization, till very few portions of the globe, perhaps, can now claim an exemption from it. There is, indeed, only a single exception in the universality of its pervadence of which I am aware. It is stated, on the best authority, "that no case of small-pox, measles, or whooping cough, has been met with in New South Wales,"*

* Evidence before the Committee of Emigration of the House of Commons.

which is the more remarkable, from the freedom of intercourse with that colony.

Not having existed in the classic ages, there could be no term for it in the Greek or Latin languages. But as some appellative designation was required for the disease, Variola was coined from either the Latin word *varius*, signifying *spotted* or *speckled*, or *varus*, a *pimple*. The vernacular title is said to be derived from the Saxon *poeca*, a pock or little pouch, and the epithet *small*, *variola minuta*, was afterwards adopted to distinguish it from syphilis, when it appeared, then vulgarly called great pox.

The first case on record of *Variola*, by this name, is that of Elfrida, daughter of Alfred of England, and wife of Baldwin the Bold, Earl of Flanders, and the next, that of her grandson, Baldwin. The one occurred in 907, and the other in 961. These cases are interesting as showing, that the disease had crept into the west of Europe at an earlier date than is generally stated. Nor, probably, were they the only instances of it. Destitute of medical writers at the time, there was no regular history of the disease, and it is presumable, that these two cases were singled out by the monkish annalist who relates them, as notable events, from the superior rank of the personages in whom they happened.

It has been usual to divide the variolous disease into two species, or varieties, according to the appearance of the eruption, the distinct or discreet, and confluent, or when the pustules are separate, or, with intervening spaces between them, and when they run into each other and coalesce, so as to form nearly an undistinguishable mass. But this is an arbitrary division, it often happening, that while the eruption is distinct in one, it may be confluent in other portions of the body. It is also wrong to characterize a disease by a single incident, however prominent it may be, and, especially, when it is fluctuating, and liable to diverse modifications. Besides which, the distinction is founded entirely on an external appearance, that has comparatively little connexion with the real pathology of the disease. As variola is attended by fever, or a general condition, either inflammatory or congestive, it seems to me, it were better to treat of it, at least as concerns practical advantages, in conformity with these views, and hence, I shall adopt such an arrangement.

Nothing very peculiar is discernible in the introductory symptoms of the first or inflammatory small-pox. Like pyrexia, generally, it commences with languor, weariness, aches in the head, back, and lower extremities, chilliness, alternated by flushes of heat, thirst, nausea, or vomiting, præcordial uneasiness, pain in the epigastrium, and some rigidity, or soreness of throat. Fever being developed, the pulse becomes full and active, the skin warm, the face turgid, the eyes slightly injected, the perspiration hurried, the tongue white, or sometimes red at the point or edges, the stomach still irritated, and betraying tenderness on pressure, the bowels costive, and the urine scanty, and high-coloured. The irritation ex-

tending to the lungs or appendages, these betray the implication, by acute or dull pain in some part of the chest, and by more or less embarrassment of breathing, according as the pulmonary substance or tissues, or the trachea, or its terminations, may be concerned.

Continuing pretty much in this way, till towards the third day, some exacerbation of the febrile state is manifested, and now confusion of mind, or even delirium may occur, or there is heavy somnolency only. The epigastrium is exceedingly tender, the vomiting more violent, the tongue very florid, the hands and feet cold, while the surface generally is hotter, with a singularly copious perspiration, emitting a very peculiar and offensive odour. Nor is the pulmonary disturbance less heightened, by an increase of the catarrhal, laryngitic, pleuritic, or peripneumonic affections. During this period, hæmorrhage from the nose is apt to occur in adults, and convulsions in children, in whom this further peculiarity may be remarked, that they perspire less, and have not in an equal degree the smell to which I have alluded. An exasperation of symptoms is usually the immediate precursor of the eruption.

Breaking out, as minute red specks, about the end of the third, or early on the fourth day, first on the face, particularly on the forehead, nose, around the mouth, then on the neck and wrists, the eruption extends over other parts of the body, the chest, abdomen, and back, lastly on the lower extremities, and is completed in forty-eight hours. From the commencement of the eruption, the fever gradually abates, and, with its completion, entirely subsides.

These red specks, at the close of the second day, become a little elevated, having a slight central depression, and an inflamed base. Towards the fourth day they undergo a further change. Now may be perceived in them, a small portion of limpid fluid, as in a vesicle; thence they enlarge, and become more conspicuously depressed in the middle. An inflamed circular margin or areola, of a rose or damask colour, surrounds each, which, the eruption being considerable, spreads and runs into each other, occasioning more or less increase of tumefaction, especially of the face and eyelids. By the seventh or eighth day, the vesicles having further augmented in size, they assume a more spheroidal shape, and begin to fill with purulent matter. It is conjectured, though not satisfactorily, that the vesicle is thus gradually converted into a pustule by the absorption of the pellucid fluid, and the secretion of pus in place of it, or by the former being changed into the latter. This suppurative process continues for three or four days, the pustules growing still larger, fuller, more yellow, and opaque, till they attain maturity, which is generally on the twelfth day, and now, from extension, lose their central depression, or at least, most of them undergo this change.

It is at this period, when the eruption is extensive, that the secondary fever, as it is called, arises, owing to the irritation of the skin, which subsides with the cause of it. There is, simultaneously with the fever, an aggravation of the soreness of throat, and diffi-

culty of swallowing the saliva and fluids of the fauces, which become very viscid, creating a constant seriatu, or hawking, or a copious salivation ensues, especially in growing persons; and the voice is hoarse, with other evidence of laryngeal irritation.

Little alteration immediately takes place in the eruption, and sometimes it remains stationary for several days. But, more commonly, a dark spot is soon discernible in the centre of the pustules, and, with this appearance, they begin to shrink, and dry away, till scabs are formed, which falling off, leave a red surface, that gradually disappears, or pits, or scars, which permanently endure. In the decline of this state of things, the same order is observed, as in the rise and progress of the eruption; first decaying on the face, and so, successively as it appeared.

Conformably to the preceding account, the career of this disease is distinguished by four different stages, the invasive or febrile, the eruptive, the maturative, the declinative or scabbing, between each of which stages, there is an interval, averaging from three to five days. This is the usual character of inflammatory small-pox, where distinct, though it is often infinitely milder, and more benignant. Even this form of it, however, is subject to great varieties in many other respects, and which are made the basis of numerous species by the nosologists. It is impossible, within my limits, to notice in detail, these diversities and modifications. No eruption whatever, in some cases, follows the fever; there are others, in which the form and contents of the pustules are widely different, and hence, the distinction of vesicular, vesiculo-pustular, crystalline, watery, siliquose, varicose, and horny pocks, &c. &c.

To these may be added other peculiarities; the first, where one or more pocks are included in another larger pock, or vesicle, the second, in which fresh pocks are formed on the tops of those previously existing, and a third, where there is only an efflorescence. These several varieties sometimes prevail pretty generally, though oftener individually, here and there a case presenting itself as a mere anomaly.

The disease being of an extreme typhoid or congestive nature, it sometimes begins with the manifestations of collapse, cold skin, pale and sunken countenance, great anxiety and oppression, and a very feeble circulation, which state may continue with little or no reaction. But more frequently, the invasion is indicated by languor and listlessness, dejection of spirits, heavy sighing, muscular soreness, and severe pain in the back and lower extremities, alternations of chilliness and flushes, great præcordial distress, and sickness of stomach. This state, which is usually protracted, is slowly succeeded by the development of fever, with a small, weak, quick pulse, unequal distribution of temperature, the head and trunk being extremely hot, amounting to even the "*calor mordax*," of the old writers, while the extremities are cool, the perspiration scanty and clammy, or the reverse, copious, and of a cadaverous odour. As the disease proceeds, it is marked by a gradual disclosure of

cerebral and nervous disorder, giddiness, disposition to syncope, heaviness or absolute coma, subsultus tendinum, sometimes convulsions, free discharges of pellucid urine and watery diarrhœa, particularly if there be not vomiting.

The state of the epigastrium is not uniform, sometimes exquisitely tender on pressure, and is often otherwise, owing to the extinguishment of organic sensibility. Nor is that of the tongue, which is florid, and apparently raw, or heavily encrusted with dark sordes.

Generally, the eruption shows itself earlier, by a day or two, than the ordinary period, while, in other instances, it is more delayed, even to the fifth or sixth day, sometimes partially appears, and recedes, producing the most deadly sickness, syncope, or stupor and convulsions. Taking place, however, the face is covered with small papular specks, which run into each other, forming a red, tumified, rugous surface.

But on some occasions, the primary appearance is that of an erysipelatous rash, or efflorescence. Little or no remission of fever is discernible on the occurrence of the eruption, and very often it is exasperated.

Great irregularity prevails in the further progress of the case. The eruption tardily advances, or reversely, very rapidly, so that the entire superficies is almost simultaneously covered. It may happen that the natural powers stop at this point, and no further effort is made towards pustulation, and the eruption becomes livid, or the whole recedes. But where it is otherwise, the pimples gradually enlarge, and fill with a thin, gleety, or darkish fluid, and rarely with yellow consistent, purulent matter. These imperfect vesicles, instead of assuming a definite figure, with a flattened surface, and central indentation, are of every shape, sometimes conoid, or sunken, with ragged edges, coalescent, or interfluent, or if there be intervening spaces they are very pallid.

Every portion of surface is, at this time, swollen, the face, the eyelids particularly, so much so, as to be closed, and the hands and feet, in scarcely a less degree. The eruption having reached maturity, the pustules are so blended, as to form one mass, or, in some parts, scarcely any separation can be discerned between them, constituting the confluent disease, which though it may occur in the inflammatory, is much more incident to the congestive form of it. Extreme exhaustion not existing, an increase with the secondary fever of all the affections takes place.

To an aggravation of sore throat, and difficulty of swallowing, are added obstructions of the larynx, and much pulmonary and cerebral disorder, from heavy venous congestions of, or effusions in, these organs. Cases of extraordinary malignity are, moreover, marked by petechiæ, vibices, colliquative hæmorrhages, especially bloody urine, with copious diarrhœa, and the pustules finally bursting, the matter escaping hardens into brown crusts, which fall off in ten or fifteen days, should life be preserved.

Of the malignant, there are as many varieties as of the more benignant disease, among which may be enumerated, the erysipelatous, the morbillous, the miliary, sanguineous, and gangrenous or putrid, so called from the exhibition of such appearances, instead of those of the ordinary eruption.—*Philadelphia Medical Examiner*, May, 1838.

MEDICAL REFORM.

We beg leave to direct the attention of our readers to the new Regulations for Medical Graduation adopted by the University of Dublin. The advantages that this metropolis affords for medical education are too well known to require comment; but it has been charged against us, that there are here no means of obtaining a medical degree on reasonable terms as to price and general education. To this point the superintendents of our University have applied themselves in the spirit of *genuine* reform, and rectifying whatever seemed imperfect, they have avoided the still more glaring errors of modern times, as to the regulation of what is termed medical education by the *certificate* system. The necessity of classical and scientific knowledge to a certain extent, including the principles of mathematics and (what is so great a desideratum in the medical world) of correct reasoning, is generally admitted; and education, to this extent at least, is rendered indispensable in our University, by requiring every medical graduate to have completed the scientific and classical study of the first two years of the undergraduate course. Persons who prefer, as formerly, to unite education in arts with that in medicine, and to graduate in both, are permitted to do so after a probation of from four years to four years and eight months, according to the exertions they may have made to keep up with the College class. Many students accordingly, who distinguish themselves as scholars, are also remarkable for their progress in medicine, and obtain a medical degree at the end of four years from entering the University. The total expense of this mode of graduation, including *all* the Collegiate and medical fees, is about £137, payable by instalments during four years. The new mode of graduation (similar *so far* to that in Cambridge) is principally designed for those candidates who wish to separate study in medicine from that in arts, and who are accordingly required to complete the *indispensable* part of the latter during two years. Candidates of this order can complete their entire education in medicine and arts in from five years to five years and eight months, and graduate in medicine after the lapse of this period from entering the University. The total expense of this mode of graduation is nearly forty pounds less than the other, and may be accomplished for less than one hundred pounds.

Some will say the *cuniculum* is not sufficiently extensive. In

this respect the Heads of the University have taken warning from the conduct of other Institutions. Instead of imposing a cuniculum burdensome as to expense, and the number of the lectures that *ought* to be attended, they have followed up the principle of the School of Physic Act of Parliament, which, without attaching much importance to the *names* of the courses, infers that seven lectures must, by a proper arrangement, be able to teach what one candidate is expected to learn. *Regular* attendance on one of each of these courses is indispensable. The candidate may acquire the rest of his knowledge as he can, and Dublin amply affords the means; but he must satisfy the Professors, on examination, that he is adequately informed on the different subjects into which medicine is divided, according to the regulations of the School of Physic Act.

It really does not appear that there is any thing more to be desired. There are ample means and inducements for classical and scientific education; but only that portion which all competent judges deem to be indispensable, is rendered so by the regulations. A moderate probation as to medical study is required, and a sufficiently searching test by examination is added. The terms, we need not say, are as low as is consistent with rendering the medical a respectable profession.

New Regulations for Medical Graduation in the University of Dublin.

“The times of graduation are Shrove Tuesday and the first Tuesday in July. The medical examinations terminate the Tuesday of the preceding week. Candidates must have previously completed their medical education, and produced a chart testifying to the details of the same and subscribed by the Registrar to the Professors in the School of Physic, as well as by the persons signing the certificates.

“Medical students may obtain the degree of Bachelor of Medicine in two modes:—

“1st. Candidates *who have graduated in Arts* may obtain the degree of Bachelor of Medicine at any of the ensuing half-yearly periods of graduation, provided the requisite medical education and examinations shall have been accomplished. The payment at entrance is £15. The fees for study in Arts, during four years, are £7 10s. each half year; and the fees for graduation in Arts, £8 17s. 6d.

“2nd. Candidates are admissible to the degree of Bachelor of Medicine, *without previous graduation in Arts*, at the end of five years from the July* following the Hilary Examination of the first undergraduate year, provided the usual education and examinations

* “The first undergraduate year may be saved by attending the October examination of that year, by a student who has entered not later than the first Monday of the July of the same year, and who has completed the payments previously made by his class.”

in Arts of the first two years of the undergraduate course shall have been completed, as also the medical education and examinations as in the case of other candidates. The fees for two years' study in Arts (besides the usual entrance payment of £15) are £7 10s. each half year.

"The graduation fees for the degree of Bachelor of Medicine are £11 15s. The Testimonium of the M. B. degree will contain the following certificate: 'Testamur sedulam operam medicinæ navasse et examinationes coram professoribus feliciter sustinuisse.'

"The medical education of a Bachelor of Medicine comprises attendance on the following courses of lectures, (of which three, at the discretion of the candidate, may be attended at the University of Edinburgh,) in the School of Physic established by Act of Parliament, provided that one, and not more than three, of the courses which begin in November be attended during each of four sessions. The courses are on—Anatomy and Surgery—Chemistry—Botany—Materia Medica and Pharmacy—Institutes of Medicine—Practice of Medicine—Midwifery (by the Professor to the College of Physicians)—Clinical Lectures at Sir Patrick Dun's Hospital during at least one session of six months as delivered by the Professors in the School of Physic—the attendance on such Clinical Lectures by the Professors to be extended to three additional months of another session, unless the practice of the hospital be certified by the ordinary physicians of the institution to have been attended from the 1st of May till the 1st of November following the session.

"The fees for attendance on the Clinical Lectures are regulated by Act of Parliament—they amount to £3 3s. to the Professors for each three months' attendance, and (provided the student be of two years' standing in the University) £3 3s. to the Treasurer of the Hospital for the first year, with a proportionate sum for any longer period. The fees for each of the other courses are £4 4s.

"The examinations for the degree of Bachelor of Medicine are conducted by the Regius Professor of the University, the Six Professors of the School of Physic, and the Professor of Midwifery to the College of Physicians.

"No further examination is requisite for the degree of Doctor of Medicine, which may be taken at the expiration of three years from taking the degree of M. B., *provided the candidate shall have graduated in Arts*. The fees for the degree of Doctor of Medicine, which entitles the possessor to the same elective privileges as the degree of Master of Arts, are £22.

"Signed,

"J. H. SINGER,

"Deputy Registrar."

"July, 1839."

Nitrate of Strychnine for Paralysis.—A child, three and a half years of age, born of healthy parents, remained apparently well

until the end of April, 1834, when, without any apparent cause, it was seized with paralysis and convulsive movements of the upper and lower extremities, and paralysis of the tongue; the expression of the face was wild, and the child had been in this state fourteen days when the author was called in; he found no symptoms of fever or congestion about the head; the appetite was good; tongue clean; bowels natural. As the child had formerly passed some worms, anthelmintics were administered, and a few lumbrici expelled, but without any relief; on the contrary, the child became thinner every day. The following medicine was now given:—

Nitrate of Strychnine, gr. i.; dissolve in
Alcohol, one drachm; add

Cinnamon water, two drachms. Three drops thrice a day.

The dose was gradually increased until the child took 30 drops or 1-10th grain of strychnine in the course of the day.

After the lapse of a few days the patient's condition was much improved, the convulsive movements declined, and the power over the extremities was gradually recovered, and at the end of six weeks the child was completely cured.—*Lancet*, from *Sieb. Journal*, vol. xvii. No. 3.

Medical Students in America.—University of Pennsylvania.—Four hundred and two medical students matriculated in this school the past session. The number of graduates was 158.

Transylvania University.—The number of students in the medical department of Transylvania University during the session 1838-39 was 211; and at a commencement held on the 11th March, 1839, fifty-one received the degree of doctor of medicine.

Dartmouth College.—From the catalogue of the officers and students of this college, issued in September last, it appears that the number of medical students at that time was 78.

Medical College of Georgia.—The class the past session numbered 60; and at the commencement held 2nd March, 1839, twelve were graduated doctors in medicine.

Geneva College.—The number of graduates in medicine in this college was, in 1835, six; in 1836, eight; in 1837, five; in 1838, seventeen.

Louisville Medical Institute.—The catalogue of the class during the session of 1838-39 shows the number of students to have been 120.—*American Journal*.

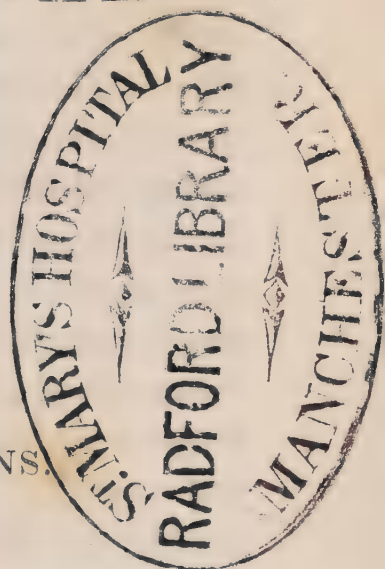
Citrate of Quina in Intermittents.—This is recommended by Professor Beraudi, because it is equal in effect to the sulphate, in doses one-third smaller; and because it is more easily borne by the stomach, and causes less congestion in the brain.—*Bulletin de Thér.* and *Schmidt's Jahrb.*

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PART I.
ORIGINAL COMMUNICATIONS.



ART. X.—*Observations on the Nature and Treatment of Acute Rheumatism.* By ROBERT LAW, A.M., M.D., Physician in Ordinary to Sir Patrick Dun's Hospital.

ACUTE rheumatism has afforded the pathologist but few opportunities of investigating either its nature or seat. It is not in itself a fatal disease, and therefore any examinations of parts affected with it have been but the accidental result of some fatal complication; or when death has been quite independent and unconnected with rheumatism. Hence it is, that we observe so much diversity of opinion to obtain on the subject both of its nature and of the tissues affected by it. With respect to its nature, some deny its right to be looked upon as inflammation, because that it has not the tendency to terminate in suppuration; because of its peculiar mobility; and because it is not amenable to the treatment of ordinary inflammation. Others again can see in it nothing but common inflammation; and even adduce cases to prove that it does terminate in suppuration.

The subject of what tissues are principally affected by it is still more a matter of controversy. Some assign to it as its proper seat, the muscular fibre; others the cellular tissue interposed between the fibres. Some regard the fibrous tissue as principally engaged. The synovial membrane has been selected as its peculiar seat, and little difference made between an affection of the joint from rheumatism, and from any common cause. The nervous tissue too has been considered the *locale* of rheumatism, and the disease thought to depend upon irritation propagated from the spinal marrow along the nerves. When the nature and seat of the disease are so little determined on, it is no matter of wonder that its treatment should share in the same uncertainty; and the consequence is, that there is no disease which has been so differently treated, and none in whose service the *Materia Medica* has been so largely and variously drawn upon.

Another evil effect of its undetermined nature is, that it has been mixed up with other affections, between which and it, we are satisfied careful examination would have detected a palpable difference. Thus, M. Chomel establishes so little difference between gout and rheumatism, as to say that gout is rheumatism engaging the small joints. We affirm that true gout and true rheumatism have as few common features as almost any two diseases, no matter how distinct from each other. We shall briefly glance at these differences. How distinct are the sensations of pain in each? Does not one affected with gout complain of the hot, burning sensation; while the sufferer from rheumatism describes his pain as a boring, twisting sensation. Is not the deep crimson hue of gout very different from the pale blush of rheumatism? And if these local characters distinguish them, how much more striking are their constitutional differences? If in rheumatism the digestive system be more deranged than in any other febrile affection, is not such derangement accidental, while in gout it constitutes the very essence of the disease? Again, when the two diseases, acting in their single point of coincidence,

leave one part to fasten on another, do they select the same organs or tissues; or when they do, do these organs exhibit the same effects from the two diseases? Are the symptoms of an affection of the heart, whether the metastasis of rheumatism to the organ, or a simultaneous affection, the same, or like those exhibited, when gout attacks it? or what affection, unequivocally rheumatic, presents any resemblance to gout of the stomach? Do we not observe the serous and fibro-serous membranes the most frequent seats of internal rheumatism, while gout, when it attacks internal parts, shows a marked predilection for mucous membranes? While we have rheumatism affecting the pleura, pericardium, the serous membrane within the eye, &c. do we not as frequently meet with the bronchial mucous membrane, the conjunctiva, the urethra, &c., the seat of gouty inflammation? We had occasion lately to observe dysentery in a gouty habit, and so unlike any form of the disease that we had before witnessed, that we felt no hesitation in charging it upon the gout, with which the patient's illness had begun. The discharges were attended with excruciating pain, and consisted of a copious bloody fluid, like water in which meat had been macerated, and emitted a most offensive fetor. Gout had left other parts which had been previously affected by it. These differences seem to us sufficient to establish a distinction between gout and rheumatism. While we admit that rheumatism has peculiarities that distinguish it from common inflammation, yet we conceive it cannot be refused its place among inflammations, when we find that a part affected by it exhibits all the phenomena of inflammation, viz. pain, heat, redness, and swelling. And although all these appearances may suddenly disappear, and not leave a trace behind, yet we do not conceive that any particular duration, any more than the want of a tendency to suppuration, is essential to inflammation. Is not erysipelas as migratory in its character, and yet it holds its place among inflammations. And surely we have no great difficulty in getting instances of inflammation as indisposed to suppuration as rheuma-

tism, and yet they are allowed to retain their place among inflammations. We quite agree with M. Chomel, that the cases which have been produced to prove, that rheumatic inflammation does sometimes terminate in suppuration, or that rheumatism is inflammation, because it sometimes terminates in suppuration, fail to establish the fact, inasmuch as these cases plainly belong to a class of diseases perfectly distinct from rheumatism, both in nature and treatment. M. Piorry observes, in *La Lancette Francaise*, Fevrier, 1833. “Le rhumatisme articulaire aigu a été considéré chez nos malades comme une inflammation des jointures. Nous rappelant que Tissot avoit noté les absces parmi les terminaisons du rhumatisme, ayant présenté à la memoire une observation rapportée par Fauchier où un rhumatisme du coude et du genou se termina par suppuration, et trois cas remarquables cités par Méfait (*Diss. Inaug. an. 1810*) où les synoviales des grandes articulations enflammées contenaient un fluide purulent; un fait observé par M. Dupuytren auquel assista M. Cruveilhier et mentionné par M. Vallerand de Lafosse, où il y avoit du pus soit dans beaucoup d’articulations soit entre les muscles et cela chez un sujet qui avoit un rhumatisme avec coexistence d’une *phlebitis*; cette observation dont parle Pinel où à la suite d’un rhumatisme il y avoit des absces entre muscles et dans une grande articulation, &c. nous rappelant avoir présenté aux élèves à la clinique l’articulation scapulo-humérale pleine de pus chez une femme qui avoit été atteinte d’un rhumatisme de cette articulation; nous etayant de l’opinion de M. le Professeur Fouchier qui d’après des faits nombreux considère le rhumatisme aigu comme inflammatoire et le traite comme tel; nous n’avons pu regarder l’inflammation des jointures comme une complication du rhumatisme mais bien comme constituant le rhumatisme lui même.” We have purposely quoted M. Piorry’s observations on this subject at full length and without translation, that there might be no possibility of mistake. We would now ask any one acquainted with pathology how far he can concur with M. Piorry in re-

garding these cases as instances of true rheumatism terminating in suppuration; and whether he conceives the treatment suited to them to be such as ordinary acute rheumatism demands. The form of disease which he has described, and upon which he rests the inflammatory character of rheumatism, has afforded us more opportunity of ascertaining by *post mortem* examination its true character than rheumatism has done. Its co-existence with phlebitis lets us into the secret of its real nature. And if this modification of disease have exhibited the same symptoms to M. Piorry, that it has to us, we cannot but question the suitableness of a plan of treatment which would deal with it as with inflammation. We suspect the disease which M. Piorry describes as rheumatism, is better known now than it was at the time he made his observations, at least it has been much more frequently observed in this country, being often met with in low fever, and after parturition. It seems more properly to have its place among that class of diseases that are characterized by constitutional irritation, such as diffuse inflammation of the cellular membrane, unhealthy erysipelas, malignant pustule, &c., in which we have not only the suppuration of the joints, but also the abscesses through the muscles as described by M. Piorry. We regard acute rheumatism then as inflammation, though mobile in its character, and without any tendency to suppuration. We look upon it as prominently exhibiting the local features of inflammation; and although necroscopic examination has not often exhibited to us the external parts which are usually the seat of the disease, at the moment when we might pronounce upon the nature of their affection, yet we have other evidence, establishing by analogy both the nature and seat of the disease. When rheumatism affects internal parts, either by metastasis, or as a simultaneous affection, is it not fair to infer, according to the known pathological principle that similarity of anatomic constitution exposes to similarity of disease, that whatever be the nature of the internal affection, the external is the same?

If then we have unequivocal evidence of the internal affection being inflammation, we conclude the external to be inflammation also. And surely a disposition to effuse lymph is as characteristic of inflammation as is a tendency to suppuration? When we have occasion to examine a fatal case of pericarditis, the effect of rheumatism, does the nature of the affection admit of a question? What are the most frequent appearances exhibited? Are they not such as occur only from inflammation? Do we not observe lymph thrown out in greater or less quantity, either floating in the turbid serum, or variously moulded on the surface of the pericardium; and in different stages of organization, according to the duration of the disease? Do we not find the same organized membrane of lymph become the seat of subsequent inflammation, and exhibiting an intense-ness of inflammation, such as we rarely observe in an original attack. And when the pericarditis does not terminate fatally, but some other disease afterwards affords us an opportunity of seeing its effects, we observe the laminae of the pericardium connected by a cellular tissue, in some cases admitting of a limited motion between them, but in others so cementing them together as to lead to the mistake of the pericardium being wanting. We would ask are these results other than those of inflammation? But although we have not ocular evidence of the actual condition of the parts affected with rheumatism, yet we sometimes can judge with a certainty little less than this—by the touch. We have at this moment under our care a case in which the knee joint is especially engaged; and were it not that the shoulder joints and one index finger were also affected, the steadiness with which the disease has kept to the knee would have thrown some doubt on its rheumatic character. In this case, which has been for some time in hospital, the subject of it has had, from time to time, rigors preceding effusion into the joint, when fluctuation became distinct; but in a few days it disappeared, giving place to the sensation of some more consistent matter, in smaller quantity, and not distending the

membrane of the joint so as to give it the globular form it had immediately after the effusion. We make no doubt that the sensation conveyed was such as pasty lymph in the joint was calculated to convey; and we believe that the opposite synovial surfaces were in the condition of giving rise to the phenomenon of *frottement*, if the same motion took place between them that does between the opposite pericardial and pleural surfaces in its production. The rapid succession of effusion and absorption in this case resembles what we see as quickly to take place in pericarditis, where the unusual extent of dull sound on percussion in the præcordial region, and feebleness or indistinctness of the sounds of the organ bespeak the effusion; while the diminution of the extent of the dulness, and proportionate distinctness of the sounds, with *frottement*, tell us of the absorption taking place, of the organ having more room to move in the pericardium, and that its surfaces are only now separated by the lymph coating each, whose rubbing against each other gives rise to this particular acoustic phenomenon.

We have just had under our care, a case of acute rheumatism, in which the dull sound in the præcordial region came on very suddenly, without any preceding or accompanying pain; and it was only when the extent of dulness diminished, that the patient complained of the inconvenience of the heart's action. The organ seemed to have more room to act, according as the effusion, upon which the dull sound depended, disappeared. The stethoscope also could appreciate the difference of the energy of its action, according to the absorption of the fluid. This effusion into the pericardium, and absorption, sometimes take place with the same rapidity as in a joint, they may not occupy more than a day or two. It is thus we have pleuritic effusion, indicated by egophony, come on and disappear within four and twenty hours. But while we have, in some instances, this rapid appearance and disappearance of effused fluid, in other cases we have a continued succession of these alternations taking place at varying intervals, and just as we find that ■

single joint may be the seat of several *reprises* of rheumatic inflammation, in like manner shall we also find the pericardium affected, so that when we were full of expectation that we had succeeded in subduing the disease, it returns with little less than its original violence.

While we believe that in acute rheumatism the effusion which takes place, whether into a joint or into the pericardium, is always the result of inflammation, we believe the inflammation is not always the same, and that when the absorption takes place so speedily as we have observed it sometimes to do, that there has been no lymph, only serum, effused.

The diminution, if not the complete subsidence of the pain in a joint affected with acute rheumatism when effusion takes place, is analogous to what occurs in inflammation of serous membranes; and if the cessation of pain be more complete in the latter than in the former, it seems to be so, because that there are more tissues involved in the inflammation in the one than in the other; and as all do not share equally in the effusion, neither do they participate to the same extent in the consequent relief.

While the inflammatory nature of the internal affection affords us analogical evidence of the nature of the external, we would make use of the same proof to decide the question of the true seat of rheumatism. When in an attack of acute rheumatism metastasis to an internal part takes place, or what is equally common, when an internal part is simultaneously affected, we would ask, which are the parts that are most frequently the seat of this internal affection? The fibro-serous membranes exhibit the most marked disposition to be thus affected, and after them the serous membranes.

We were going to observe, that the pericardium held the first place amongst the internal parts, but experience has satisfied us, that endocarditis is still more common as a consequence or complication of acute rheumatism, than pericarditis. When then we consider the anatomic constitution of the endocardium and pericardium, we find the first, at least the part of it that

forms the valves, (the part especially subject to rheumatism,) to consist of a membrane analogous to the synovial membrane of the joints in anatomical and physiological characters, and of the tendinous expansion of the cordæ tendineæ between the duplication of this membrane. This tendinous expansion has its corresponding anatomic elements in the tendinous structure which enters into the formation of the joints. To the mitral valve having so much more of this tendinous structure in its composition than the sigmoid valves of the aorta, we would ascribe its being so much more frequently the seat of rheumatic affection than they are. The pericardium exhibits in its compound constitution its serous lamina corresponding to the synovial membrane, and its fibrous lamina to the fibrous elements of the joints. Rheumatic iritis also affords some aid in deciding upon the tissues involved in rheumatism when affecting external parts. Were it the serous membrane alone of the iris that predisposed it to rheumatic inflammation, any other portion of the membrane of the aqueous humour would equally be subject to be affected, which we do not find to be the case; as we do not find all the endocardium to be in the same degree prone to be affected from rheumatism, but that the portion which forms the valves is especially so. From observing the superior frequency with which the compound structures, whether fibro-serous membranes, as the pericardium and endocardium, or such a structure as the iris, composed of serous membrane and muscular structure, take on the rheumatic inflammation, we infer that it is these same structures that are affected when rheumatism attacks external parts, that in fact the internal affections are types or counterparts of the external.

Of the simple structures affected by rheumatism in the internal parts, serous membranes are the most frequent. We had had under our care several years since a case of acute rheumatism. The affection engaged several of the joints. The patient complained of pleuritic pains, and exhibited the stethoscopic signs of pericarditis and endocarditis. We had the indi-

vidual under observation for years. He never lost the *bruit de soufflet* which indicated to us the endocarditis. Death from fever gave us an opportunity of proving the existence of adhesions, by loose cellular bands between the pleuro-pulmonalis and costalis, in the situation to which the pain had been referred during the attack of rheumatism. The laminæ of the pericardium were connected by cellular membrane which admitted of limited motion. The mitral valve was thickened, and formed a hard resisting ring all round its attachment to the heart. The opening was not narrowed, and the valve seemed sufficient to close it. The individual did not suffer much inconvenience from a continued strong action of the heart, and never had a spitting of blood.

We deem it unnecessary to enter into any lengthened detail on the local symptoms of acute rheumatism, as we have already adverted to them in asserting the inflammatory character of the affection; still we feel that our description of the disease requires a more full consideration of them. Although the skin of the joints affected with acute rheumatism is sometimes more red and injected than usual, yet this is far from being constant, it is even sometimes of a dead white and blanched appearance. When it is red it never assumes the deep damask hue of gout. The affection of the joints which terminates in suppuration, and whose symptoms seem to us to entitle it to be placed among the diseases marked by constitutional irritation, exhibits the skin of a delicate peach-blossom colour.

There is no modification of disease that is attended with more intense suffering than acute rheumatism. None in which the patient's expression more significantly portrays the misery he is enduring; when you approach his bed he shudders from the apprehension of the slightest touch or motion. The weight of the bed-clothes is too much for him to bear. He lies perfectly motionless; and if irresistible necessity obliges him to make any motion, he performs it as if the entire body were of one piece, or as if every joint of it were ankylosed. When

we consider the seat of rheumatism we learn the reason of this great suffering. The tissues involved are those which in their healthy and normal condition occupy the lowest place in the scale of sensibility, but which, when diseased, exhibit a most marked exaltation of it. In this particular the synovial resembles the serous membranes, which under ordinary circumstances are almost insensible; but when inflamed their inflammation is characterized by most acute distressing pain. The same may be said of all the other tissues which enter into the composition of the parts affected with rheumatism; ligament, tendon, fibrous tissue, &c. all undergo the same modification of sensibility from disease. The sensation of pain is differently described by different patients; one describing it as a twisting of the parts; another as a boring sensation; another as a dragging (*tiraillement*) of the parts. Perhaps these various descriptions of the painful sensation may be explained in this way, that as the character of the pain varies in the different tissues, and the tissues affected by rheumatism are numerous, in one individual, one tissue may be more affected than the rest; and in another, a different tissue: and whatever be the tissue most affected, its peculiar modification of sensation will be the predominant one, and will be the one felt and described by the patient.

The swelling of parts affected with acute rheumatism depends either upon effusion into the parts, or upon a thickening of the parts; or perhaps it would be nearer the fact to ascribe all the swelling to effusion; but this effusion differing in character, sometimes consisting only of serum and quickly disappearing; at other times composed of serum and lymph, the latter afterwards as much identified with the parts as we observe it in pleuritis. We before observed how an affection of the knee joint afforded us an opportunity of distinguishing by the touch between the two effusions. It is the lymph modified and changed in its character that constitutes the permanent thickening of the structures] engaged, and deprives them

of their original suppleness and mobility, although the joint may to the eye present no deviation from its natural form. In the examination of persons affected with disease of the heart, a peculiar stiff unyielding feel of the wrist, accidentally observed when feeling the pulse, has declared to us a previous rheumatism, and thus thrown light on the nature of the affection of the heart.

The extent of the swelling depends on the parts where it occurs. It is most remarkable in the knee joints, and imparts to these joints a globular form. The back of the hand and fingers are frequently the subject of swelling, as well in rheumatism as in gout ; nor do we allude to that particular form of the disease, designated by Haygarth nodosity of the joints, but simple effusion into the sheaths of the tendons, proceeding to the fingers, distending the parts, and depriving the fingers of their taper form ; giving to the hand very much the shape of the wooden hand employed by glovers to clean gloves upon, those clumsy, shapeless fingers but ill represent nature's perfect workmanship. The bursæ too, as well as the joints, are the seat of effusion in acute rheumatism. We have often observed the bursæ at the point of the elbow to contain fluid in these cases.

We have remarked that in most cases where the effusion takes place, the pain either ceases altogether or is greatly lessened ; this, however, is by no means constant ; we have even heard patients say, that it was not until the swelling had taken place that they began to feel pain. Although the effusion relieved the synovial membrane, yet by stretching and distending the other parts it caused pain in them. This appeared to us to explain the phenomenon.

Unquestionably the larger joints are more frequently affected with rheumatism than the smaller ones ; yet the latter are sufficiently often affected with rheumatism, and the former with gout, to prevent our pronouncing upon the nature of the affection from the size of the joints engaged.

The alternations of heat and cold being the most frequent

cause of the disease, men, from the nature of their occupations being more exposed to such alternations, are more frequently the subjects of it than women.

The knees are most frequently affected in the first instance among men, then the other joints in their turn. We commonly find that the shoulder joints are the last in order of succession to be engaged, and that when all the intensity of the disease has subsided they are complained of.

The nature of the occupation of the individual, or some other peculiar circumstance, will direct the disease to some joints rather than to others. Thus, the fingers are generally affected in washer-women, who constitute the great portion of female rheumatic patients that come under our care. We have met with the disease affecting the sacro-iliac symphysis in females after delivery, owing no doubt to the effect produced in this joint during pregnancy. A hurt or injury will fix the locality of rheumatism. Children seem to enjoy a peculiar exemption from the disease. The most remarkable feature of the disease is its erratic or migratory character, shifting from joint to joint. It is this peculiarity that has led nosologists to assign to it its special character and separate place, distinct from common inflammations. It is from this peculiarity that it derives its danger as involving important and vital organs. Another characteristic of the disease is its proneness to repeated attacks and returns.

The constitutional symptoms of acute rheumatism are very various, sometimes scarcely appreciable, at other times exhibiting the intensest type of inflammatory fever. We generally find that the severity of the constitutional symptoms bears a certain relation to the extent of the local affection, or to the number and size of the joints engaged. The fever is commonly of a marked sthenic type; however, we have sometimes observed it of a typhoid character; this seemed rather to depend upon the debilitated condition of the patient previous to the attack of rheumatism, than upon the essentially typhoid character of the

fever. The pulse is commonly full and strong. The skin exhibits a condition very different from its ordinary condition in other febrile affections, in being bathed in profuse acid perspiration. We sometimes find it hot and dry. The tongue is generally loaded with whitish fur, but should the subject of the rheumatic attack be of a gouty habit, or have been previously liable to bilious or digestive derangement, the tongue is covered with yellowish fur, and in this case the urine exhibits the usual marks of such gouty or bilious derangement in its lateritious deposit. The patient's expression bespeaks the intensity of his suffering. If he gets any sleep, it is in short, broken snatches, from which he derives no refreshment. He lies on his back in a state of the most complete helplessness, incapable of the least motion. The fever does not seem to abate any of its severity during the day. If the fever run very high the patient is sometimes delirious. We have now described what is designated rheumatic fever; but between this extreme and the hardly appreciable constitutional disturbance sometimes existing, there are many degrees. Sometimes the constitution does not seem to respond to the local affection.

We before observed, that no disease had been so variously treated as acute rheumatism; none upon whose cure so many different medicines had been employed. The diversity of opinion on the nature of the disease sufficiently explains this circumstance. Those who regard it as common inflammation have treated it as they would any other inflammation, seeing nothing special in it, and therefore not requiring any special modification of the antiphlogistic treatment. This view of the disease, and corresponding treatment, were adopted by Sydenham. His views and practice have been pursued, and still more decidedly in Paris by M. Bouillaud, whose treatment of acute rheumatism consists in repeated bleedings (*par la saignée coup sur coup*) as long as any trace of inflammation existed. M. Broussais regarding acute rheumatism as local inflammation, treated it as such by the application of leeches to

the affected parts. Wherever an inflammatory appearance presented itself, there were the leeches at once put. Others again, neither admitting nor denying its inflammatory character adopted different modes of treatment not directly antiphlogistic, or at least less decidedly so than either general or local bleeding, viz., sudorifics, purgatives, &c. Tartar-emetic having been so successfully employed in other diseases procured for it a trial in acute rheumatism. They who refused altogether its inflammatory pretension, and who were controlled by no special indication, treated it altogether empirically, going the rounds of almost the entire *Materia Medica*. Mercury, opium, bark, colchicum, digitalis; other preparations of antimony besides tartar emetic, viz., James's powder, white oxide of antimony; aconitum, iodine, &c. &c.—all these medicines have, in their turn, been used in the treatment of acute rheumatism.

Although Sydenham practised free and copious bleeding in acute rheumatism, he afterwards altered his practice, and ended in almost leaving the disease to run its course, merely interfering with diluent drinks. M. Bouillaud has revived the practice in Paris, and continues to bleed largely while the fever exists. M. Chomel, in expressing his disapprobation of this mode of treatment, says that it debilitates the patient, and thus renders him more susceptible of the effects of different morbid causes: that although it relieves the pain, it leaves behind a thickened condition (*un empâtement*) of the joints, which interferes with their motions, and prolongs indefinitely the convalescence. Others have thought that copious and often repeated bleeding has led to metastasis of the disease to internal parts. We will remark no further upon this practice here, than that if an inflammatory appearance of the blood be made the rule to guide us in repeating its abstraction, it is a rule that will lead us to an unsafe length; it will in fact lead us to drain our patient of all the blood in his body. Few, very few, have been the instances where we have not seen the blood buffed in acute rheumatism, and many, very many, have been those in which

we have not felt ourselves warranted in bleeding more than once.

Although we generally find that a certain ratio exists between the degree of constitutional derangement and the extent of local lesion in acute rheumatism, yet we pretend not to assert that this ratio is constant, nor do we regard the fever as symptomatic of the local lesion, and, therefore, should not expect that leeches applied to the local inflammation would reduce the fever. We have seldom made the experiment, but when we have, we have not had much reason to congratulate ourselves on its success. One great objection with us to trying the cure of acute rheumatism by means of leeches is, that in most cases the operation would require an exposure to cold that must be attended with risk. Of the value of leeches, however, we shall speak presently. Of the treatment by purgatives we shall only observe, that were we more certain of their effect in speedily subduing the disease, we should be slow to employ a mode of cure which must inevitably produce great inconvenience and pain from the helpless state in which the patient generally is. We generally deem it enough to empty the intestine of its contents, which might keep up irritation, and thus increase the fever.

That sudorifics should have been employed in the treatment of acute rheumatism seems somewhat strange, when we consider the ordinary condition of the skin in the disease, how frequently it is bathed in profuse perspiration. It must be admitted, however, that in some cases the skin is hot and dry, without any disposition to moisture ; yet these cases are so few in number as not to warrant sudorifics being placed among the remedies in the disease. In most of the cases where we have observed this dryness of the skin, it was in the midst of an epidemic fever, whose type the rheumatic fever seemed to assume. Blood drawn in such cases did not generally exhibit the decided inflammatory appearance peculiar to the disease. Of all the remedial agents employed in the treatment of acute rheu-

matism with a view to act on the skin, none has been more used than the warm bath. Independent of the objection that lies against all sudorifics, that they are not required, the great inconvenience of the bath from the helpless condition of the patient, and the suffering from any motion, would constitute a great objection to it, even if it were useful; but this is so far from being the case that we have invariably found that it has added to the sufferings of the patient; he has always complained of his pains being worse after it. Sometimes it is not easy to mark the line of demarcation between acute rheumatism and chronic, when the former is passing into the latter; and from the efficacy of the bath in the latter we have been induced to try it at this stage, but have generally found no benefit from it, and regarded its effect as a kind of test of the disease not having passed into that stage when the treatment suited to the chronic form may with advantage be substituted for that which the acute demands.

We have given mercury a fair trial in the treatment of acute rheumatism, and have not found that when we employed it alone, we could say that the cure was accomplished in a shorter period than if it had been confided to the unassisted efforts of nature. We must allow, however, that in the thickened condition of the joints, especially observable in the wrists and knees, and which thickening we attributed to effused lymph, there we have found mercury eminently serviceable. The matter to which the thickening is owing, is soon removed, and the joints recover their power of motion. We would here repeat an observation which we have elsewhere made on the use of mercury in the treatment of inflammation, that it is especially applicable when and where the tendency of the inflammation is to throw out lymph. This fact we had a recent opportunity of verifying in the case of rheumatic inflammation, involving all the structures of the knee joint, but especially the synovial membrane. After freely leeching and stuping the parts, we employed mercury till the mouth became sore, yet it did not ap-

pear to make any impression on the disease. At a more advanced stage of the disease, however, and when the synovial membrane seemed to the touch to be thickened, we returned to the mercury, and with most decided advantage.

Opium has been variously used in the treatment of acute rheumatism. Some have depended upon it alone, having premised one or two bleedings. Dover's powder has long occupied a high place among the remedies in rheumatism, but has had its utility compromised by sufficient discrimination not being exercised as to the precise time of its employment. It is a most valuable remedial agent in subacute rheumatism, but is quite inapplicable to the stage, where we already have too relaxed a skin. Where the pain has been very distressing, and by its intensity seemed to keep up the fever, we have exhibited opium largely, and with decided benefit. Of course we resort to it to procure sleep, and we give a decided preference to Batley's sedative over every other preparation of opium.

Of antimonial medicines, the only ones we feel ourselves called upon to notice, are tartar emetic and James's powder. The antiphlogistic properties exhibited by tartar emetic, in the treatment of pneumonia, recommended its use in acute rheumatism. We gave it a fair trial, but did not find it to answer our expectations. When the condition of the skin would have warranted our employing a diaphoretic, we did not use James's powder alone, but combined it with calomel and opium. And some instances did occur to us, when the circumstances of the cases prevented our treating them in our ordinary way of treating the disease, and in which we adopted this combination, but found the case very prolonged and tedious.

We would remark of bark, as we have done of other remedies in acute rheumatism, that it is a very valuable medicine in the treatment of the disease, when the proper time is chosen for its exhibition: or if due attention be paid to the cases which require it, and are benefited by it. Some cases in which the constitution is much dilapidated, and in which we would not

dream of any active depletion are well treated with bark or quinine, from their very commencement. It is very much with this disease as it is with erysipelas; we have had, at the same moment, and lying beside each other, two patients affected with erysipelas, one we treated with tartar emetic, and the other with quinine and wine, and both successfully. Individual constitution will require a modification of treatment. While some cases of acute rheumatism, commencing with fever of a typhoid type, will require tonics, such as bark, from their earliest stage; others will begin with a strong sthenic type, but soon assume this character, requiring tonics. The usual combination of bark and sulphuric acid will be found advantageous, as it will control the excessive and debilitating perspirations. In those cases in which we conceived tonics indicated, we have not been deterred, by a furred tongue, from exhibiting quinine, nor did we find that in such cases the medicine disagreed.

Digitalis has been employed in the treatment of acute rheumatism. We conceive its proper place, in connexion with the treatment of the disease, is when the heart is involved, otherwise it seems to us only directed against a mere symptom of the disease.

Colchicum has been extensively employed both in the treatment of acute rheumatism and in gout, and like many other medicines we may say of it, "laudatur ab his, culpatur ab illis." The evil effects that in many cases attended the use of the eau medicinale naturally raised a prejudice against colchicum, a prejudice which has operated so strongly upon some practitioners, as to deter them from using it. Some untoward results from the use of colchicum* itself, have added to this strong feeling against it. When we consider to how much un-

* M. M. Caventon and Peletier, from analysis of the bulb of colchicum, considered it to owe its activity to veratrine, a vegetable alkaline base, white, pulverulent, without smell, and extremely acrid, producing violent sneezing when applied to the pituitary membrane, and not soluble in water. A small quantity adminis-

certainly the medicine must of necessity be subject when prepared from the bulb, whose virtues are so completely dependent on the season at which it is collected, and how it must often be almost impossible to insure its being gathered at the proper season, we can readily acquiesce in the opinion of those, who think it too dangerous an agent for a practitioner to have any doubt about, and therefore it would be as well left out of the *Materia Medica*. But as we have the preparations from the seeds, free from this source of uncertainty, and also have in them all the value of the medicine, we are not reduced to the alternative of its complete proscription. We have now for several years employed the wine of the seeds of colchicum in the treatment of acute rheumatism, and with most decided advantage, nor have we ever experienced any of the unfavourable effects which have been observed to attend its use. We have never exceeded two drachms in an eight ounce mixture, and of this given an ounce every third hour. We have always observed the precaution of adding a small quantity of tincture of opium to the mixture, in order to reconcile it to the stomach. While we have experienced the value of colchicum in the treatment of acute rheumatism, we must admit that we rarely, if ever, found it productive of benefit, if its use had not been preceded by one or two bleedings. We made the experiment of trying it alone, but came to the conclusion that it was not competent to cure the disease. Our experience on this point agrees with that of almost all practitioners with whom we

tered to a cat destroyed life. On examination after death, the top of the œsophagus was found intensely inflamed, but no where else.

M. M. Geiger Hesse and have extracted from the seeds of colchicum what they have designated colchicine, which differs from veratrine in many striking particulars. Colchicine forms crystals; it excites no sneezing when applied to the nose; it possesses but feeble alkaline properties, and is soluble in water. It is very poisonous, a small quantity killed a cat, the examination of which, after death, exhibited the stomach and bowels violently inflamed, with effusion of blood. The œsophagus did not share in the inflammation.

have spoken on the point. We have further remarked, in reference to colchicum, that we have not been able to connect its efficacy in allaying pain, &c. with any palpable action whether on the bowels, or kidneys, or skin, &c. ; and often when the benefit from it has been most striking, such action has been least observable.

Although in noticing the different modes of treatment of acute rheumatism, and the various remedies that have been employed, we have anticipated much of our own treatment, yet we shall detail it systematically as we have now pursued it for many years, and with a degree of success that we feel to warrant our recommending it.

It is our invariable practice to bleed in acute rheumatism, in its earliest stage, if there be any febrile excitement, unless there be something peculiar in the case to contra-indicate it. Of course the extent to which we carry the depletion is regulated by the height of the fever and the strength of the patient. If a single bleeding fail to make a decided impression on the disease, and the patient's strength allow it, we repeat it very soon. Convinced of the inflammatory nature of the disease, bleeding recommends itself to us on the double ground of lessening the inflammation, and of rendering the system more susceptible of the operation of the other remedies we may think fit to employ. We before observed, that we cannot take the appearance of the blood as our guide to repeat the operation of bleeding in this disease, for that it retains its inflammatory aspect much longer than we could with safety persist in its abstraction. By much the most protracted cases of the disease that have been under our care, were such as had something peculiar in them that prevented our bleeding ; or such as had existed for some time without any active energetic means having been employed ; or when the constitution had so little responded to the local lesion that we did not feel ourselves warranted in resorting to general bleeding. In these last cases, in the progress of the disease the necessity for bleeding not unfrequently arises,

but the operation is never attended with the same salutary results as when it is performed at first. To adduce an instance illustrative of tediousness from our not being able to bleed in consequence of the peculiar circumstances of the case, we would allude to a female who was attacked with the disease from exposure to cold air, to relieve her from syncope from excessive uterine hæmorrhage during her confinement. When she came under our care she was still so much under the effects of the loss of blood that we could not think of bleeding. This was the case in which the sacro-iliac symphysis was affected. The case continued more than three months in hospital, in consequence of the temporizing treatment we were obliged to pursue; nor was it eventually completely successful, as it terminated in ankylosis of the knee. We must join issue with M. Chomel on his assertion that bleeding produces *empâtement* of the joints; we believe the contrary to be nearer the fact, that its neglect more frequently produces it. As the type of the fever is sometimes of a low typhoid character, (nor do we now allude to the arthritis with purulent effusion into the joints, which we observe in the progress of some low fevers and other modifications of disease,) and as the subject of the disease is sometimes of a broken down habit, in such cases, of course, we do not think of bleeding.

Simultaneously with bleeding we direct colchicum. A drachm of the wine of the seeds in a six ounce mixture, according to following formula, is what we begin with:

R. Aquæ Menth. Piperitis, \bar{z} vi.

Vini Seminum Colchici, \bar{z} i.

Tinct. Opii, gutt. xx. Misce.

Sumat unciam tertiâ quâque horâ.

When we wish to act upon the bowels, which we only do when they are torpid, and when we would empty them of their contents to prevent this additional source of irritation, we either direct an ounce of sulphate of magnesia to be added to the preceding mixture, or order a purgative injection.

If gouty symptoms, such as acidity of the stomach, bilious derangement, &c. be present, we add a drachm of carbonate of magnesia to the mixture.

When we would act on the kidneys, two or three drachms of spiritûs etheris nitrosi are added to it.

In some cases, when the skin has deviated from its ordinary condition in this disease by being dry and hot, we have directed the following mixture :

℞. Aquæ Menth. Pip. ℥iv.
 Aquæ Acetat. Ammon. ℥ii.
 Vini Semin. Colchici, ℥i.
 Tinct. Opii, gutt. xx. M.

We direct an opiate at night, and, as we before stated, very much prefer Batley's sedative.

Although the patient's condition generally is such as to cause every movement to be attended with pain, yet such attention should be paid to his linen being changed, as that he incur not the risk of chill from remaining in clothes soaked in clammy perspiration.

Although we are far from confiding as much in the use of leeches in acute rheumatism as M. Broussais, yet we value them very much as a subsidiary means. In cases where there is no constitutional movement, where the local lesion seems insufficient to provoke general symptoms, then we content ourselves with leeches alone ; but as these cases are comparatively few where there is any extent of local lesion, so we rarely trust to leeches alone. But we have derived much advantage from their use in alleviating pain, and relieving the swelling and tension of the parts. We had a case in which the back of the hand and all the fingers were much swollen and painful. We made the experiment of applying leeches to the metacarpo-phalangan articulations of two of the fingers alone ; they completely removed the swelling, and relieved the pain of the two fingers and corresponding part of the back of the hand, leaving the other parts in *statu quo*. We have just experienced their beneficial effects

when applied to the shoulder joint. The disease had already affected the other joints that it usually attacks, and now engaged the shoulder, in consequence of which there was complete immobility of the arm. They quite removed the pain and restored the motion of the arm. When the disease has less of its migratory character than ordinarily, and when it seems to fix itself on one or more joints, then leeches are most useful. In some cases, as we before observed, lymph seems to be effused; and becoming organized, itself becomes the matrix of inflammation, and that more readily than an original structure. This is analogous to what we observe in pericarditis, in which the lymph originally thrown out, and now become the connecting medium between the laminæ of the pericardium, is often the seat of inflammation, announced by pain; which although it will in many cases yield to leeches, &c., yet we have seen so intense as to destroy life. After death we have found this lymph most highly vascular, and blood effused copiously into its substance.

We have found nitrate of potash a valuable remedy in some cases. In fact we could bring forward some of the most severe cases we have ever met with, which have yielded to one or two bleedings and to nitre alone.

M. Chomel, in his clinical lectures on acute rheumatism, reported in *La Lancette Francaise*, August, 1834, says, “le rhumatisme articulaire aigu quelle que soit la medication employée ne se termine jamais avant le vingtième jour.” We can with as much confidence assert, that the average duration of the most aggravated cases of acute rheumatism which we have had to treat, and which we have treated by bleeding and colchicum, has been from ten days to a fortnight; and we have further observed, that the more severe the symptoms have been at first, the shorter time did the disease last, because we have then treated it more vigorously. When the treatment occupied more time, it was either because the case had been in other hands, where bleeding had not been employed: such a case have we under treatment at this moment. The subject of it, a

female about 32 years of age, had been ill three weeks before she came under my care in hospital. No depletion had been used. The skin was bathed in profuse clammy perspiration; the pulse extremely rapid and small; the heart's action sharp and irritable; the intellect quite astray, so that she could give us no account of herself: the wrists and knees were swollen and puffy. The prostration was extreme. Here we were obliged at once to begin with stimulants and tonics, and gave her quinine and a small quantity of wine, from which she has improved very much. Or when the disease has not at its commencement exhibited such decided symptoms of fever as to warrant active measures, but did so later in its progress than in fact it had begun under a sub-acute form. Under these circumstances the energetic steps afterwards resorted to, do not usually produce the same measure of relief as if they had been more early employed, the disease having grown into a habit, and, therefore, requiring more trouble to be removed. Another cause of the prolonged duration of the disease beyond the average limit, is its being complicated, as for instance, with either pericarditis or endocarditis.

When we state the duration of acute rheumatism, or rheumatic fever, under the treatment by bleeding and colchicum, to be from ten days to a fortnight, we would not be understood to mean that this is the duration of the acute stage, which at the end of this time passes into the chronic. No; we feel persuaded that when acute rheumatism does pass into the chronic state, it is, in the great majority of cases, due to the great neglect of active early treatment.

When the severity of the symptoms of the early stage has, in some degree, abated, as it generally does after one or two bleedings, if the condition of the skin be not relaxed, we direct a full dose of Dover's powder at night, while we give the colchicum during the day.

In cases where the digestive system appears to be deranged, which is commonly the case in gouty habits, we direct small

doses of pil. hydrargyri, or make a bitter mixture the vehicle for the colchicum : when the acute symptoms have altogether disappeared, when in fact the disease has yielded, leaving behind it stiffness of the parts that had been the seat of the rheumatism, and general debility, we then direct warm bath and quinine, or some other preparation of bark.

We shall detail a case of as marked symptoms as we have generally met with, and exhibiting our ordinary treatment.

Anne Magee, aged 24, married, a week previous to admission into hospital, was suddenly attacked with rigor, succeeded at night by heat, restlessness, thirst, loss of appetite, headach, &c. Next day she broke out into profuse perspiration, but without any relief to her symptoms. At the same time she was seized with severe pains and swelling of the elbows, wrists, knees, ankles, and joints of fingers.

At present she complains of acute pains of the above enumerated joints, and extending along the limbs. The sensibility of the affected parts is so great, that she cries out on the slightest pressure ; they are very much swollen, red, and hot. She has pain also in the lumbar region, but not so severe as in the other parts. Her countenance bespeaks the greatest distress and suffering. Her face has a peculiar greasy appearance. She is bathed in profuse perspiration ; pulse full and frequent ; tongue coated with thick yellow fur in centre ; bowels torpid. She gets no sleep.

Venesectio ad uncias duodecem.

R Aquæ Menth. Piperitid. ʒvi.

Vini Seminum Colchici. ʒi.

Tinct. Opii. gutt. xx.

Carbonat. Magnes. ʒi. M.

Sumat unciam 4tis horis.

Enema purgans vesperi. Haustus anodynus opiatu horâ somni.

July 29. Blood very much buffed and cupped. Fever a degree less. Joints very much swollen, red, and painful.

Repetatur mistura Colchici. Hirudines articulis maxime dolentibus. Haustus anodynus horâ somni.

30th. Swelling, redness, and pain of joints, to which leeches had been applied, much diminished; anxiety of countenance less; face still retains its greasy appearance; skin hot and dry; pulse 120, full.

Repetatur mistura Colchici.

R Calomel. gr. iv.

Pulv. Jacobi veri, gr. vi.

Extract. Opii aquosi, gr. ii.;

Fiant pilulæ quatuor; una quartis horis sumenda.

Leeches to the joints to which they were not before applied.

August 1. Leeches have relieved the pain and swelling of the joints; heat of skin less; countenance has lost much of its anxious expression; bowels confined.

Adde Mist. Colchici, ℥vi.; Sulphat. Magnesiae, ℥i.; et st. unciam 3tis horis, donec alvus responderit.

We continued the colchicum mixture until the 7th, when we reported the pains and swellings entirely gone, and the patient complaining only of debility, for which we directed quinine.

The treatment of this case occupied nine days, and we have seldom seen more joints at once affected, more suffering, and higher febrile excitement.

We shall detail one more case in which the constitutional symptoms were not so very marked.

John Murphy, aged 26, labourer, admitted into hospital August 3rd. A fortnight since, from exposure to cold while in a heat, he was seized with acute pain in the left ankle, and shortly afterwards in right wrist; at the same time he was hot and restless, lost his appetite, and complained of thirst: he had no rigor. While suffering from the pains he bathed in the sea, after which they became much more severe. At present left ankle and right wrist are swollen, red, hot, and excessively painful. An erysipelatous redness extends from the inflamed ankle up the leg; he also has pains in both hips, but much less

severe. Expression of countenance extremely anxious ; skin of face of a dead white colour, and of a greasy appearance ; surface generally rather hot and dry. Pulse 80 ; bowels confined. Pains prevent sleep.

Venesectio ad uncias duodecem. Mist. Colchici et Sulphat. Magnes. ad alvi solutionem. Haust. anodynus opiatus horâ somni.

4th. Blood slightly buffed ; countenance much less expressive of suffering ; pain and swelling of joints much less. Pulse 70, full and strong. He can now move his joints, which he could not do before.

Hirudines quatuor carpo. Mistura Colchici.

5th. Leeches have very much lessened the pain and swelling of wrist.

7th. Right knee considerably swollen, with distinct sense of fluctuation ; very painful, hot, and red.

Hirudines duodecem genu affecto. Fomentationes postea. Reprtr. mist. Colchici.

8th. Pain has entirely left knee, leaving some swelling behind ; all the other pains are much better.

℞ Calomel. gr. vi.

Pulv. Scillæ, gr. iii.

Extract. Opii aquosi, gr. iss.

Ft. pilulæ quatuor. Sumat unam quartis horis.

The swelling of the knee soon disappeared, and in a very few days he became quite well.

In such a case as the preceding, when the constitutional symptoms are not very marked, when the circulation is not much disturbed, we have always found the advantage of adopting a treatment beyond the apparent exigency of present symptoms ; it has in general very much abridged the duration of the treatment.

When we do not feel warranted in resorting to decided anti-phlogistic means from neither the local nor constitutional symptoms being very severe ; or when the constitution appears

to take no notice of the local affection, which may consist in a thickening of the parts apparently dependent on effused lymph; when the disease either has assumed a sub-acute form from the beginning, or has quickly passed into this form, we may at once proceed to use mercury. We find the thickening (*empâtement*) of the parts disappear under its use; and in this form of the disease, or at this stage of it, we experience warm baths especially useful.

The value that we have derived from the empirical remedy, the Chelsea Pensioner, has led us to adopt it as our electuarium antirheumaticum; and we can say of it that there is no single medicine nor combination that we have found equally useful in sub-acute rheumatism. The following is our formula.

℞ Sulphuris, ℥ i.
 Bitartrat. Potassæ, ℥ ss.
 Pulv. Rhei, ℥ ii.
 — Guaiaci, ℥ i.
 — Nucis Moschatae, ℥ i.
 Mellis ℥ iv. M.

Sumat cochlear. medium ter die.

When we find it to act too strongly on the bowels, we direct a drachm of Dover's powder to be added to it.

In this form of the disease, and when the condition of the system is the very reverse of febrile excitement, when it in fact seems to require to be excited, the tinctura guaiaci ammoniati is a very valuable remedy. We have seen it act like a charm in lumbago.

We shall not allow ourselves to go farther into this subject, as we only proposed to ourselves to treat of acute rheumatism. We confess how difficult it is to say when acute rheumatism ends and chronic begins, the transition from one to the other is often so very insensible. The treatment which we have detailed is what we have followed for several years, and we may say with a measure of success equal to any other treatment of any other disease. We arrogate not for it unvarying success; we

have not often found it to fail, but we have sometimes found it not to accomplish the cure as speedily as at other times. But still, in the great majority of cases, we have found it to bring the cure within the limits of a fortnight. When we reflect on the ordinary duration of the disease, generally from a month to six weeks at least, and the sufferings to be endured in that time, a mode of treatment which promises a speedy cure recommends itself on strong grounds for adoption.

We shall elsewhere advert to the complications of acute rheumatism, especially that of pericarditis and endocarditis; we would here, however, allude to one which has been very partially noticed, either by those who have observed on rheumatism, or by those who have remarked on the disease with which the complication occurs, we refer to scarlatina. We have observed acute rheumatism so often to occur in the course of scarlatina that we cannot regard the occurrence as merely accidental. Nor is it alone rheumatism affecting the joints that we have observed, but also both endocarditis and pericarditis associated with it. We have also seen endocarditis come on in the course of scarlatina; which proved to us that this modification of disease was not in all cases the result of metastasis, because in the case to which we allude, there was no trace of rheumatism affecting the joints.

Before we conclude our observations on acute rheumatism, we would allude to four remarkable cases which came under our care within the last year. To one of them we have already alluded, and remarked, that if it were not that the shoulders and one finger were affected at the same time as the knee, upon which the intensity of the disease was concentrated, we should have questioned the propriety of regarding it as a case of rheumatism. It exhibited none of the characteristic disposition to change place of this affection. In two cases the wrist joints were effected, in one the elbow joint, and in the fourth the knee joint. In all those were present all the phenomena of inflammation in a very aggravated degree, viz. intense pain,

superficial redness, swelling, and increased temperature. Nor were these confined to the parts immediately about the joints, but in some cases extended for some way to the muscles of the respective extremities ; and in the case of the elbow joints, the periosteum, both of the humerus and the ulna, seemed to be affected. What characterized all the cases was the peculiarity with which they resisted treatment. In all the cases the patients at the time of the joint becoming affected, were either labouring under other disease, or under some moral depression. In one case the patient had been affected with pneumonia, for which she had been actively treated by bleeding, &c. ; here the wrist was affected. Another patient was labouring under typhoid fever when the elbow joint became affected. The two other cases, although not the subjects of actual disease at the time they were seized with the local affection, yet were in a state of great moral distress. They were both advanced in pregnancy. In one the wrist, in the other the knee and metacarpo-phalangan joint of the right index finger were affected. In none of the cases from the state of the pulse, and from the physical prostration, as well as moral depression, could I entertain the question of general bleeding. My treatment consisted of repeated application of leeches, stupes, fomentations, poultices to the affected joints, and calomel and opium exhibited till the gums became tender. When the leeches removed the acute pain we then applied blisters and counter-irritation in various shapes. The pain being very severe in some of the cases, and there being a good deal of constitutional irritability, we administered opium largely. Very often the leeches removed the pain for a short time, but its return required their being applied again. In one case 160 leeches were applied to the knee. In all the cases we found that the patient had to be submitted to the influence of mercury twice before she seemed to derive any real benefit from it ; an effect which we attributed to the remedy being more suited to the disease in its advanced than in its early stage. It was in fact when the swelling or effusion took place that seemed

to be its most seasonable period of application ; its benefit then was much more palpable. We found it expedient here, as we have in many protracted cases of rheumatic pericarditis, especially in scrofulous habits, to resort, and rather early, to tonics, such as quinine, &c.

Three of the cases completely recovered, and one is now under my care and in the course of recovery. Of the three the duration of the shortest was ten weeks. The most obstinate is the one at present under cure ; she has been in hospital nearly four months. The first case of this kind that came under my notice was several years since, when a young woman who had come from the country to service in town, received a pinch in the arm from a child that was under her care. She thought she felt it more than she ought; the arm became livid and black. Her general health became deranged at the same time, when she was also seized with pain in the knee and swelling of the parts about it. I employed with it the same treatment as in the other cases. It exhibited the same obstinate resistance to treatment, and ended in imperfect or incomplete ankylosis of the joint. The two first cases of the four to which we have alluded, when they left us exhibited such immobility of the joints affected that we despaired of their ever recovering motion ; however, by friction and use they did attain to a degree of motion little inferior to what they originally enjoyed. We make no doubt that the condition of the synovial membrane in these cases is analogous to that of the pericardium, when from inflammation it has thrown out lymph that becomes a connecting medium between its laminae, which would be oftener identified were it not for the motion of the organ. In the same way the motion of the affected joint interrupts the adhesion of the opposite synovial surfaces, which otherwise would undergo this false or incomplete ankylosis. As we have seen membranous bands (*brides*) of variable lengths extending from the opposite pericardial surfaces, so have we seen similar productions between synovial surfaces.

While the modification of disease to which we have just adverted, resembles closely in its constitutional symptoms the affection of the joints where suppuration takes place, yet it differs essentially in this particular, that let it be protracted no matter how long, it has neither a tendency to pass into suppuration, nor to the absorption or destruction of the cartilages, an effect which we have witnessed in the other affection, when the disease has only existed a few days. Such a case lately came under our notice in a young boy who was under our care for scarlatina, from which he seemed to be recovering, when suddenly a considerable fulness and redness of all the parts in the cervical region came on. The sterno-clavicular joints were swollen and red; the pulse was small and extremely rapid; the abdomen very tympanitic: he had delirium at night; and sank in five days from the first appearance of those untoward symptoms. The early symptoms of the scarlatina did not prepare us for such a result. The *post mortem* examination exhibited all the cellular tissue of the neck infiltrated with a sero-gelatinous fluid, with purulent matter diffused among the muscles. The sterno-clavicular articulations contained a thin, greenish, purulent matter, with the ends of the bones quite rough and denuded of their cartilage.

We deem it a point of especial importance to draw attention to the fact of the likelihood of these cases of diffuse inflammation, purulent arthritis, phlebitis, &c. being confounded with or mistaken for acute rheumatism, a mistake which must lead to serious practical mischief as leading to treatment the very reverse of that required. To prove this we would refer to a case of glanders in the human subject, which occurred in Hotel Dieu, at Paris, in September, 1838; reported by M. Rivet, and given in full detail by my friend Dr. Bigger, in No. XLV. of Dublin Journal for July, 1839, from Froriep's Neue Notizen.

Dondelignère, the subject of the case, was admitted into hospital, September 29, with countenance red and flushed; pulse quick, and tolerably strong; skin hot, and head aching.

He particularly directed attention to his right shoulder, which was very painful and much swollen; motion, and handling the anterior wall of the axilla increased the pain. The skin in this part appeared of its natural colour, and the subcuticular cellular tissue did not appear puffed. On the 30th the patient exhibited the same symptoms as before. *Diagnosis, acute rheumatism of the shoulder joint*, for which venesection was ordered, with three cups of a ptisan of borago and nitre, and one grain of opium in two pills. There being no alleviation of pain, in the evening twenty leeches were applied to the shoulder. On the 2nd October he was bled again. On the 4th the right arm was very much swollen at the elbow, the skin red, and in the region of the olecranon there was a black spot as large as a dollar. He was again bled, and had the arm enveloped in a linseed meal poultice.

On the 5th symptoms nearly the same, except pulse very feeble. Pustules, surrounded by a circle of inflammation like varioloid pustules, appearing on different parts, and a discharge of brownish mucus from the nares, led M. Rivet to recognize the error he had committed in diagnosing as a case of acute rheumatism, what was really a case of glanders. On his consulting with MM. Husson and Blandin, they agreed in regarding it as a case of glanders, and advised the patient to get a decoction of bark, containing a drachm of the extract, Bordeaux wine, and soup. The case terminated fatally. Dondelignère had been about glandered horses. This case completely proves the point to which we would draw attention. In it we observe a particular view taken of the case, and a corresponding treatment adopted, but when subsequent symptoms exhibited its real nature, a diametrically opposite plan of treatment is resorted to.

A case not unlike Dondelignère's occurred to us a short time since in a female, who, in addition to the symptoms of pleuropneumonia, complained of most distressing pains all through her limbs and neck. Nothing relieved her pains; her throat

became sore, and she expectorated a greenish purulent matter. After a few days, a few pustules with inflamed areolæ appeared on different parts, some of them were hard and acuminate, others were mere elevations of the cuticle, containing bloody fluid. We now saw the disease in its real character; deglutition became difficult, and she was continually endeavouring to clear her throat of the greenish purulent matter, which came away in great abundance. The larynx and upper part of the trachea seemed to be the seat of the irritation. The spots assumed a more decidedly gangrenous appearance. Erysipelatous patches presented themselves on different parts of the body; a large tumour, with sense of fluctuation, but without discoloration of skin, formed on left tibia. Her pains were very distressing; the pulse became very rapid and small; she now had a muttering delirium, with tympanitis and diarrhœa, and soon expired. Examination of the body exhibited ulceration of the larynx and top of the trachea. A portion of the substance of the lung was broken down into a broken jagged condition, infiltrated with pus. The right pleura pulmonalis was raised into a number of pustules closely resembling the original appearance of those upon the surface of the body. The tumour on the tibia was a collection of greenish purulent matter. This case resembles glanders in the human subject in some particulars; and on investigating we learned that a glandered horse was in a stable very near the patient's residence, but that she had had no direct communication with it. It is to the pains to which we would especially direct attention, as occurring constantly in such cases, and nearly as often leading to mistake as to the real nature of the disease. We feel no hesitation in saying that such mistake has been made in the diagnosis of some of the observations of M. Bouillaud, in his *Traité Clinique des Maladies du Cœur*, for instance; Observation 42:—"Homme de 30 ans. Rhumatisme articulaire aigu. Fievre tres violente, anxiété. Mort le dixième jour après le debut du rhumatisme. Rougeur presque generale de la membrane interne du cœur surtout aux valvules; caillots fibrineux et

albumineux dans les cavités du cœur. Rougeur et epaississement de la veine saphene interne. Pus dans les articulations." Pains in the knees caused the case to be regarded as one of acute rheumatism, and to be treated as such. The patient was blooded six times, and had leeches applied to the knees twice.

Whoever has observed how frequently phlebitis is complicated with inflammation of the joints, terminating in suppuration, (a complication often coming under the notice of the midwifery practitioner,) will be able to judge how far the case to which we have alluded deserves to be set down as a case of acute rheumatism. Observation 41 belongs to the same pathological category. "Homme de 31 ans. Douleurs dans les membres. Plus tard symptomes d'inflammation pulmoniare. Mort assez rapide. Rougeur de la membrane interne des oreillettes et caillots fibrineux dans les cavités du cœur. Pus dans les veines des membres. Fausses membranes dans les divisions de l'artere pulmonaire. Plusieurs foyers purulens dans les poumons." The individual was admitted into hospital for the pains in the thighs, "qu'on prit d'abord pour une neuralgie." In the reflections on the case, the question is asked, "les douleurs que le malade eprouvait dans les membres inferieures quand il fut admis à l'hopital etaient elles rhumatismales?" We believe, that they, in no respect, resembled the usual pains of acute rheumatism. We would make this distinction between them, that while the pains of acute rheumatism are confined to the parts inflamed, in these cases the pains are felt in other parts more sensibly than in the affected joints; besides the nature of the pain is different. Moreover, we have never seen, in acute rheumatism, the pathological phenomenon exhibited in this observation, "les foyers purulens dans les poumons," which we have frequently observed in that class of diseases which we consider to be wrongly mixed up with it, a confusion which must lead to injurious practice, as the type of this class of disease is almost invariably of a typhoid character, and such as will not bear active treatment.

ART. XI.—*On the Position of the Placenta or Afterbirth in the Womb, and on the Expansion and Contractions of the latter Organ during Pregnancy and Parturition ; being a Reply to a Review from the Britain-street Lying-in Hospital, upon the Author's Opinions on these Subjects.* By HUGH CARMICHAEL, A.M., Member of the Royal College of Surgeons in Ireland, and one of the Surgeons of the Coombe Lying-in Hospital.

IN the number of the Dublin Medical and Surgical Journal for January last, I submitted a paper to the Profession, containing certain reflections which occurred to me respecting the present received opinion as to the manner the afterbirth was disposed of in the womb during gestation and parturition ; and which reflections, as I then observed, impressed me very much with the idea, that strong grounds existed for questioning the truth of our being correctly informed in these particulars.

I stated in that communication, that with such impressions upon my mind, I was led, in several opportunities afforded me at the Coombe Lying-in Hospital, to investigate this matter ; and that in a series of dissections that presented themselves at that establishment and in its neighbourhood, of females who had died at or near parturition, I found the placenta, not at the fundus, as now described, *but low down on the posterior wall.*

The suggestion that its position in these cases gave, naturally led me to further investigation, and the result, as it appeared to me, went entirely to establish the fact, that the posterior wall of the womb, low down, was the true position of the organ towards the close of gestation.

The museums of Dublin, so far as I could reach them, all, in their preparations on the subject, bore testimony in its favour ; the stethoscope *never*. Here let me observe, that when I said *never*, I said it in that sense which every candid mind would take it in, and I am certain there are not three individuals in

the Profession, would seek to pin upon its literal acceptation the success or downfall of my reasoning upon the subject, or think I meant it strictly in that sense. I shall correct the expression, however, to accommodate all: the stethoscope then, in an average of at least 98 out of 100 cases, testified against the fundal situation, and pointed out its real one to be somewhere in the vicinity of the iliac fossæ; and above all, the examination of the secundines, carefully delivered, in a series of not less than 300 cases, within a period of about three months, at the Coombe Lying-in Hospital, with exceptions not greater than as above stated, told the same tale. Such were the evidences upon which I went, and such were the proofs, or I shall say supposed proofs that led me, perhaps prematurely and unadvisedly, to come to the above conclusion.

I confess, however, I was induced to think I had made out a case, and more than a *prima facie* case, to warrant me in doing so; and being impressed with the opinion, that from these facts, the lower part of the womb, posteriorly, was proved to be its proper position towards the *close* of gestation, and the fundus, or somewhere near it, being its admitted position *at the first formation of the mass*, I was hence led further to hazard an opinion which appeared to me also a rational one, as to how this apparent change of position occurred.

I could not, as I then stated, suppose, that the afterbirth absolutely shifted its position upon the internal surface of the womb, leaving the part it was first implanted on; as in that event there would be an interruption to its being in the constant and steady communication with the organ, which was indispensable for its office. I maintained, therefore, that on whatever part it was first implanted, on that identical part did it remain throughout pregnancy, and it struck me that the only possible way to account for the phenomenon under consideration was, that the enlargement which the womb undergoes during pregnancy, or I believe I would be more correct in saying, subsequent to the afterbirth being implanted on it, was mainly conducted on its

anterior wall, or I shall say, anterior section ; and I confess at this moment, after the most dispassionate consideration of the subject, I am utterly at a loss to imagine, if the two points above stated be correct, how otherwise it can possibly be explained.

I could not suppose or imagine that the womb, in its enlargement, enlarged by a general rising up of the fundus, and a distention of it in all other directions, as I believe, is the present opinion ; because, if that were the case, it must evidently have borne up along with it the after birth, and this latter must, therefore, at every time throughout gestation, up to its very close, be found at the fundus ; but as this is not the case, at least so far as the evidence I resorted to gave testimony on the subject, I came to the conclusion I have stated upon the matter.

But although I contended, that, from these reflections, I was justified in thinking the expansion of the womb went on chiefly on its anterior wall or section, (for otherwise I could not explain the matter, and I believe without such a supposition explanation is difficult,) I by no means said that the greatest expansion was there ; on the contrary, I expressly declared that this, “the greatest expansion,” was to be found *at the fundus*, my words are (p. 452) : “at the fundus the womb expands to a much greater extent than at any other part.”

Having, as I imagined, laid rational grounds for what I have here spoken of, the question naturally occurred to me, what was the object of nature in this arrangement ; for it is evident that where such an almost universal regularity of its position was to be observed, it must have been with a view to some intended end ? It appeared to me then, that the after-birth or placenta was an organ of the utmost importance to the preservation of the child, and that the peculiar office it performed was such as required rest, or perhaps I should say, would best be performed when at rest, or most out of the reach of disturbance. Now it struck me that if the placenta were placed, and continued throughout gestation, at the fundus of the womb, where the

greatest expansion goes on, and on the supposition that this expansion took place as now imagined, that for reasons to be presently mentioned, this rest could not possibly be there enjoyed by it; whereas in the position I found it in in such an overwhelming proportion of instances, if my method of accounting for how its apparent sinking on the posterior wall of the womb were correct, in that position, and as it were by that manœuvre, would this rest be *best* secured; to the consideration of these two points, and no more, did I go, namely, the placenta at the fundus, under the effect of the present opinion respecting the changes that part of the womb is supposed to undergo—and at the lower and back part of it.

Such is a short summary of the views contained in my paper of January last; and I believe I am safe in saying, the opinions expressed in it met with the approbation of a considerable number of the Profession: there were some, however, it appears who dissented from them; and finally, a reply to it, and intended as a refutation of the doctrines it contained, was published in the Number of this Journal for July last, from the Britain-street Lying-in Hospital, after it had been first read and fully discussed before the Obstetric Society lately established at that Institution, and in the presence of the medical officers of the hospital.

The objections contained in this review against my views on this subject may be resolved into two heads; firstly, conclusions drawn from what have been stated to have been my own expressed premises, laid down by myself in my original paper, but which certainly are not my statements; and secondly, arguments arising generally upon the subject, which are deemed to be fatal to my theory—decisive against it.

Before I proceed, however, to consider the force of these objections, I must first observe what I have already done elsewhere; namely, that it is the duty of every person reviewing the works of another, if he deem it proper to allude to any of the statements of that work, to confine himself, particularly in his quotations

from it, to the real expressed passages which he objects to, or purports to comment upon. If he should consider these passages "admit of a construction" which will be at variance with the conclusions his author has arrived at, it is but fair that he should state he is showing how such can be maintained, and that that is the point he is aiming at; but I am certain I shall be supported by every person, when I say, that there is nothing more to be censured in a reviewer, when alluding to the passages of an author's work, or more calculated to excite, and justly, displeasure in the mind of him reviewed, than to do it in a manner that will lead readers to suppose such are the real written passages contained in it, if they be not, and particularly if grounds be thereby laid for holding his writings up to ridicule and contempt, though the reviewer may afterwards, when the author so treated remonstrates, excuse himself on the plea, that what he said was deducible from it, and that it was in that light he meant it; and I will go farther and say, that the writer who finds himself so misrepresented, and every reader whose opinion is set right upon the matter, have every just reason to think, that he, the writer, has been most unfairly dealt with, and not with the courtesy or respect he was entitled to, or might have expected. That I have reason to complain of this in the review of my paper from the Britain-street Hospital, and Obstetric Society, I think I shall be able fully to show.

My original paper, in January last, was drawn up, as I have already stated, with a view to questioning the present received doctrine of the placenta or afterbirth continuing throughout pregnancy *at the fundus of the womb*; and I endeavoured to sustain my opinions upon this subject, by the observation, that the expansion of the womb during pregnancy was greatest *at the fundus*, and that if the placenta were situated where such expansion was going on, and also that that expansion took place as is now supposed, that the surfaces of the two organs could not *there* keep pace together in their growth, and that the consequence would be, during gestation, "*disturbance*" of the placental func-

tion, and “*probably*” partial detachment. I next went on to consider what the consequences, in my opinion, would be, when the contractions of labour set in, and I stated, as regarded them, that the first series of these contractions must constringe or lessen the uterine vessels going to the placenta, if at the fundus, and that these contractions going on and increasing in force, the placental function must ultimately be extinguished, (obviously in consequence of the obliteration of the above vessels from *permanent* uterine contraction,) if the organ itself were not detached altogether, and that early in the process of parturition. I beg to observe again, that I made these statements on the supposition of the placenta continuing throughout pregnancy *at the fundus* of the womb, and also on the supposition of the fundus enlarging by a general expansive rising up of it, pushing the placenta up before it, and further of the contractions of the womb going on first at the fundus, and then progressing over the organ in all directions, as now believed.

To my opinions, so supported, this review has taken certain exceptions, and argues these exceptions on certainly very infelicitous grounds ; it declares, that I have said, if the placenta be *in any other part* of the womb, save the lower and back parts, that the consequences will ensue which I have strictly confined to its being at the fundus, and under the above circumstances ; and further, in addition to this, adds, that I asserted *hæmorrhage* would follow *during labour*, when the placenta is so affixed *to any other part of the womb* ; and having thus then given these as my statements, or if it be more agreeable, having quoted me so carelessly as to give room to the reader to imagine them to be mine, a series of cases are brought forward to shew the absurdity of my opinions, in none of which was the placenta situated at the fundus, but on the walls of the womb ; and because the labour was in them effected without the placenta being detached, and without hæmorrhage, “the child still in utero,” these cases are thought to be destructive to my paper, and are brought forward as such.

Now if I *had* said that the consequences of the placenta being affixed to the anterior part or walls of the womb, would have been detachment, instead of confining this remark, as I did to the fundal position, and that only as probable ; and if in addition to what I did say, I further declared the gross absurdity, that with placental detachment, *hæmorrhage* would accompany labour in these mural positions of it, while the child was fully in the womb ; there is not one question upon the subject but these cases would have upset such doctrine at once, and also have proved on my part, as it must on the part of any person who could be found entertaining such a thing, an utter ignorance of the first principles of midwifery, in stating how such could possibly take place ; but as I never have made any observation of the kind, nor any thing that would bear such a construction, (at least such was never intended,) I trust I will be excused in protesting against these statements being attributed to me, in the strongest possible manner ; and I do say, that inasmuch as they are calculated to exhibit a writer on midwifery, capable of doing so, in the most ridiculous point of view, that it should be well looked into, in the first instance, to see if he has been guilty of such absurdity, before he is brought most unjustifiably before the Profession, on a charge of that description.

I did not in my paper say, that if the placenta were attached on the anterior wall, or laterally, that it could not there be accommodated, or that the expansion there was too great for that purpose ; I said nothing about these parts, my remarks in these respects were confined to the fundus, and that under the circumstances mentioned. The veryest tyro in the art is aware, that in placenta presentation, the placenta is often principally on the anterior wall, and still supplies the child often up to the birth, and that the fatality in such cases to the latter probably arises, among other matters, from the interruption to the function which takes place shortly before the child can be born, in consequence of the hæmorrhage. The partial placental presentation is well known to every one, and is often principally on

the anterior wall, and it may and does happen that after the mouth of the womb has dilated somewhat, the membranes will spontaneously break, the head descend, and pressure being so effected, all will go on well, and a living child be sometimes born without interference ; but will any one deny that this is an improper situation, or adduce rare cases of this kind as an argument against those who would point out a better position for the mass, and submit a series of rational grounds to prove it ? The child will sometimes present by the arm or the foot, and by management will be saved ; the breech will also sometimes present, and often without interference the labour goes on well, and the infant is preserved. But will any one say, on that account, these are proper presentations, or bring them forward as a proof to shew the absurdity of Doctor Denman's doctrine, as to the head being the one that is *the best* ? It has been stated, that my paper went to establish the necessity there was for the placenta being in a situation where it would be free from disturbance : it did ; but although it shewed, and I still think satisfactorily, to any unprejudiced mind, that that freedom from it was *greatest* low down on the posterior wall, under the circumstances I mentioned, where every person, even my reviewers, must *almost* invariably find it, still I did not at all say that this was the only place where it could preserve its function by any means ; there are others where it may, but less decidedly so ; and as I confined my remarks as to disturbance amounting to the decided prejudice of its office to the fundus, and that under particular circumstances, it is rather unfair to take me now to the anterior wall or sides of the organ, which I did not speak of, in order to disprove my opinions on that score, and there must indeed have been a great lack of valid cause for complaint against me, when so untenable a trifle is resorted to. Nature is not generally satisfied with where she *can* be accommodated, but where she can be *best* accommodated, and if we sometimes find her in the former position, it is not to be raked up against those who have pointed out the latter, as examples of their failure, or of

how unnecessary their work has been. I believe, since the appearance of my essay, attention has been very much drawn to the examination of the secundines; and I believe I need not observe what tale they (I shall not say in every instance) tell, inasmuch as one or two exceptions may be found in the hundred; but for those that cavil with my doctrines, let them, if they can, deny this assertion, as to the proportion of instances in which it is found low on the posterior wall, and if they be forced to *admit* it, or what is the same thing, be silent on it from an unwillingness to admit it, let them then say, either that Nature had no object in view, in thus almost so invariably placing the placenta in that situation, or if she had, let them give a better solution of it than I did; and further, let them show how the apparent change of position I have above alluded to, can otherwise take place than as I have accounted for it. We know well the great resources of Nature, when deviations take place from her usual ways; I need not here give examples of it; and if any person be disposed to produce instances of this fact, is it to be taken as disproving the general principle? such a thing, I do say, is cavilling, not scientific discussion.

In fact my essay went simply to an exposition of what I considered was the mode Nature proceeds in, when in her usual way; not either professing to explain how any aberration from that way was affected, and the resources she could fall back upon when under such circumstances, or denying the possibility of such things occurring. The writer upon the symmetry and order which Nature so *universally* observes in the formation of the female human pelvis, cannot be questioned upon the truth of his statements, because that very same Nature sometimes, by a process of her own engendering, (rickets,) warps that symmetry upon which he treats, and even still effects her object sometimes in that warped state; nor will public opinion entertain the catching of him upon the word "*universally*," because, sometimes, in disease, departures from it are met with; to say, therefore, that my paper went to establish the *necessity* of the pla-

centa being deposited in the posterior and lower part of the womb, and the attaching to it a meaning that, according to it, in no other place would the object of Nature possibly be accomplished, and that hæmorrhage and a still-birth must necessarily result from its being in any other position, and which has been endeavoured to be done, is obviously taking me in a way never intended by me ; and I am surprized how those who fancied this to be my intention, could think such an essay *deservedly* attracted professional attention, or be found making preliminary observations in any way complimentary to the writer of it.

But having disposed of the question with respect to the effects of such a position of the placenta as regards the child, let us now turn our attention to it in another way not heretofore considered, namely, as it may affect the case of the mother—a new view of the subject. Every obstetrician knows, that the proper and healthy attachment of the placenta is of such importance to the mother, that it often involves her very existence ; it must be detached fully and at once, not partially, or hæmorrhage, frequently fatal, is the result. What is the cause of hæmorrhage when the placenta does not come away ? Partial detachment. And does not Nature seem to have endowed the womb with powers within itself of completely detaching the mass unaided by art ; that is, in ordinary cases ? Now I would ask the question, when the hand is introduced into the womb to take away the placenta in cases of its retention, to effect what Nature seems, contrary to her usual way, here to be unequal to, where is it the placenta is found ? is it not in this very position where my reviewer from the Britain-street Hospital found his placentas in. Where is the placenta in those dangerous cases of hour-glass contraction ? In fact, in all such cases, is it not found in some place different from where I have pointed out *its proper* one to be. Will any person tell me, that has attended to the position of the placenta by the means I have pointed out, that out of every hundred cases, in ninety-seven at least the place at the close of gestation is not where I have located it ; and has

any person who has been obliged at any time to take away a placenta, ever found it there, *low down at the back part*, or where *has* he found it? is it in this latter situation? Where is it that morbid adhesions are found? And what is the inference from all this? why that the placenta is always (now I hope the word *always* may not be fastened on) fully and effectively detached when in my position, but most generally not so when out of it; in fact, that Nature there is astray. It may be said I am now changing my point, explaining matters differently from how I did. No, I am giving additional proofs of the correctness of my doctrine; what I have said as to the quietude such a mode of proceeding, on the part of Nature, insures the organ, must also arise from it, as well as what I now point out: it cannot be otherwise; yes, and if I might hazard a further matter for consideration, one which I think deserves the attention of the obstetric pathologist, it may probably be, that some of those cases of abortion, that occur without any apparent assignable cause, at certain periods of gestation, might owe their cause to some such mal-position of it. I do not retract what I have said in my original paper, I still think it correct; I now along with it give this additional cause for such being the object of Nature.

But let us proceed to the consideration of this review from the Britain-street Hospital; and, first, I shall lay before the reader the passages alluded to from my paper by the reviewer and those which purport to quote these passages, in order that it may be seen whether I am correctly represented or not, and whether cases based upon the data assumed by the reviewer can at all apply to my doctrines; and for that purpose the more clearly to do so, I shall set them out in two contiguous columns.

*Statements from the Coombe
Lying-in Hospital.*

“Admitting, however, that
during gestation there should

*Statements attributed by the
Reviewer from the Bri-
tain-st. Lying-in Hospital.*

“Admitting the original
position of the placenta to be

be a perfect freedom from untoward consequences, when the contractions of labour set in, I cannot see how they will not occur, and in the most decided manner.

“ These contractions, as I have already mentioned, are supposed to commence *at its fundus*, and from thence progress downwards, lessening the surface they act upon as they proceed, and increasing in power with each succeeding pain ; the first series of them, therefore, must, by contracting *that part* to which the placenta is supposed to be affixed, in the first instance *lessen* or *constrict the uterine* vessels upon which its utility chiefly depends, and thereby impede or interfere with its office to a certain extent, and the contractions going on increasing, they must *ultimately extinguish* its functions if not detach it altogether, and that very early in the process of parturition.”—p. 454.

Again, “ Let any person form an opinion as to what the state of the placenta must be *respecting the impeding of its office*, or even of its attachment to the womb, when the

at the fundus, Mr. Carmichael denies that it continues in that relative position throughout gestation, and *endeavours* to prove the result of such an arrangement should be, a partial separation, *followed by hæmorrhage* during the growth, or, *at all events, during the contractions of the womb.*”—p. 344.

Again, “ Mr. Carmichael’s arguments resolve themselves into three heads : 1st. In healthy and natural pregnancies, the placenta is never implanted on any part of the uterus save the inferior part of the posterior wall. 2nd. If the placenta forms its attachment *in any other situation*,” [here I beg attention to the words ‘*any other situation*,’] “ its growth cannot correspond to that of the womb, and early separation and its attendant circumstances must follow. 3rd. *In the latter case*,” [attachment in any other situation,] “ even though it should

capacity of the organ is lessened to the extent it must be when the head has fully entered the pelvis in a labour.”—pp. 454, 455.

Again, “That such must necessarily be the effect of the contractions upon the placenta, *extinguishing its function* early in labour, and depriving the child of the benefit it derives therefrom is obvious, *if these contractions be performed in the way I mention*, and the placenta be *at the fundus* of the womb. The statement is *by no means overdrawn*, or for the purpose of answering particular views; we find the same thing put forward by some of the French writers, and which I shall beg to set out here.”—p. 455.

Again, “If the placenta then be *at the fundus* of the womb, it is situated at the most expansive part, it is therefore, as well during gestation as labour, exposed to all the evils its unchangeable surface must give rise to.”—p. 478.

Again, “In a word, according to this theory, the placenta, in every stage, from its

able to maintain its adhesions uninjured throughout pregnancy, the consequence of the uterine contraction, when labour begins, must *inevitably* be its detachment, *followed by hæmorrhage*; in fact, according to him, such an untoward occurrence will form as truly an instance of *unavoidable hæmorrhage* as when the placenta is placed over the mouth of the womb.”—p. 346.

Again, “But laying aside theory, let us inquire, is Mr. Carmichael correct in his position, that the placenta in natural pregnancies is always placed low down on the posterior wall, and that its being implanted *in any other situation* must, *ex necessitate*, during the growth of the uterus, or *at least during its contraction to expel the foetus*, cause premature detachment and consequent hæmorrhage.”—p. 353.

formation to its end, is in a constant state of disturbance, must be exposed to *interruption* during pregnancy, and at labour to suppression by detachment and *compression*, long before it ceases to be required.”—pp. 478, 479.

When the reader inspects the two foregoing columns, and compares what I *have* said in my original paper with what I would *appear* to have said, I think he must admit I have reason to complain, and that even if it were intended only to say that such was *deducible* from my statements, this has not been put forth in a way that would lead one to think so.

Now the two misstatements I complain of here, are 1st, that I am represented to have maintained, if the placenta be implanted *any where* upon the womb but the lower and back part, that its growth cannot correspond there with that of the womb, whereas I have only said that is the case *at the fundus*; and 2nd, that I am stated to have said, that when it is in any other part, the contractions at labour must produce detachment and hæmorrhage, whereas I never spoke of hæmorrhage in any place except when the placenta was *somewhat* high up on the posterior wall; even when at the fundus I have not said it will follow, because if the placenta were detached there, it would be by permanent uterine contractions.

However, these are the two statements the reviewer seizes upon to prove, as he terms it, “the fallaciousness” of my doctrine; and having attributed these sayings to me, he proposes giving cases in support of his opposition, based upon them. I shall beg to quote the following passage to show this:—

“But laying aside theory, let us inquire, is Mr. Carmichael correct in his position, that the placenta, in natural pregnancies, *is always* placed low down on the posterior wall, and that its

being implanted in any other situation must *ex necessitate* during the growth of the uterus, or, at least, during its contractions to expel the foetus, cause a premature detachment and consequent hæmorrhage. These propositions I shall consider together ; and I submit, that if I succeed in bringing forward cases in which the placenta was *otherwise* affixed—*otherwise* affixed, and yet no hæmorrhage occurred before the birth of the child, I shall have gone a great way in sapping the foundations of the author's interesting but fallacious theory of the mode of contraction of the uterus."—p. 353.

I trust I need go no further to shew the value that is to be set upon the cases, as against my doctrine, which are based upon the supposition of the foregoing statements ; there is one thing, however, in them which I wish particularly to direct attention to, namely, looking for hæmorrhage while the child occupied the womb, the placenta being on its sides ; why for hæmorrhage to occur, the placenta must be somehow detached, but the presence of the child necessarily will prevent the diminution of the womb on the wall where the placentas were in these cases ; how then could it be detached ? and is it not a sad hardship on my part to have such silliness attributed to me, or is it more a matter of wonder that they seriously entertained such a matter at the Britain-street Hospital, instead of at once ridiculing me on it, if they could find me saying so.

But the cases—we must come to the cases : I do say, not one of them are in point. I spoke of the placenta only in two situations, the back and the fundus ; in the cases they are all on the wall, chiefly the anterior, and therefore do not apply to any thing I have said, and as looking for hæmorrhage from detachment, partial or otherwise, while the child was in the womb, this I can only say, I never even hinted at, nor can I see how it could possibly occur.

Let it not be said, that if these cases be examined it will be found that every part of the womb (certainly, save the fundus) was shewn to be covered by the placenta from one or another of

them, and therefore that what I said was upset, because no disturbance, no detachment took place in these situations. The latter I have shewn to be absurd, "the child in utero," and will *demonstrate* it when we come to analyze the sixth case ; and with respect to the former, it is only, as I have said, an instance of the cautious providence of Nature, in supplying herself with resources, when by accident or otherwise she is thrown from her usual way ; and it will be also remembered, I never spoke about it there : as well might a physiologist say, that the main artery of a limb was useless, because if it be tied or rendered impervious by disease, the circulation goes on by anastomosis ; this latter, no doubt, is a fact, but is it not better conducted by the system of a main trunk, or will the exception disprove the general rule. I only told a plain story of what I observed from dissections, and the testimony from the stethoscope and the secundines, and what struck me as the cause for such being the case, but I did not want to say I had found out what *alone* would render the entire process of conception successful, or seek to stultify the skilful professional reader, who knows well the placenta is sometimes on the anterior and lateral wall of the womb, into the supposition, that Nature here tied herself up, in a way she is almost never known to do elsewhere, in leaving herself altogether without a resource in case of need ; nor do I think examples of such lateral position can at all bear upon the question, unless it can be shewn that I said what has been attributed to me about hæmorrhage and other positions ; the fundal position, I think, will form an exception, if the expansions and contractions go on as supposed, and if a case be adduced, shewing such a one from secundine observations and length of labour, which I think material, so inconsistent is it with my plain mind, that it would go (regard being had to the three questions I have asked as to place, apparent change, and object of nature) to prove with me that the fundal expansions and contractions do not go on as now supposed.

In two of these cases part of the placenta extended high up,

but where was the rest of it then ? low down certainly ; in one, six inches at least below its top, and in the other it went down to the very os ; now I do say, if a case be brought against my explanation respecting the effect of its being at the fundus, it should be *all* at the fundus, or dome or vaulted arch, if these terms be more expressive, and what is clearly meant by the present opinion as to how it is placed, or supposed to be placed, upon the fundus ; and not to bring forward a case to disprove my assertion in that respect, where it was partly within the expansive dome and partly not ; yet this is what is sought.

Further, I say, there is but one of these cases (the third) which can be relied on as giving any satisfactory evidence upon the matter—that is, as to the true placental position ; the hand introduced into the womb, when the latter has undergone the great diminution it does to expel the child, cannot give admissible testimony as to how the placenta was placed when the viscus was in a full state of expansion. In a disputed point like this, the hand in the womb, when turning, or the secundines, are the only true proof, particularly where the contractions in question are contended for.

Neither can a point, very much relied on by my reviewer, at all apply to the dispute at issue, or be admitted as in any way invalidating my doctrine, viz. that where the placenta was found on the anterior wall by the hand introduced for its removal, that it must have been at one time at least, on the fundus, and so have been detached, &c., in accordance with my statement. If I am to be tried upon my own ground, give me the full benefit of it ; when I spoke of detachment at the fundus, I did it on the supposition of the contractions there going on in the way they are supposed, from all points in the circumference of a circle to its centre ; but if the placenta *passed* the fundus, it must have done it according to my views, and the contractions, therefore, being there similar to those on the anterior wall, or somewhat so, (rectilinear,) and not the true, or supposed true, fundal centripetal contractions spoken of, they must have

influenced the placenta and its attachment only, in the same way they would if in the latter position, or nearly so ; and, therefore, by no means admit of application in the way they are meant to do by the reviewer. But having parried my opponent's thrust in this respect, let me now try one at him : suppose we examine the matter according to his notions of uterine changes. The fundus with him is always the fundus, therefore he cannot read any of his cases against me in support of his views of fundal contractions, for he has not produced one case in which the placenta was fully and decidedly upon the fundus, or dome, or vaulted arch, if these terms be preferred, and which is certainly necessary in order thereto ; as matters are now, it is clear, he wishes first to establish, that according to my doctrine, the placenta in some of his cases was *one time* at the fundus ; and having done that, he then wishes to change the scene, and try by the placenta there, not my theory which brought it there, but his own, which, under the circumstance, it has nothing whatever to say to.

Let us now to the sixth case, and show, how sad the mistake was to consider the points connected with it to be so conclusive against me, in fact, how they vanish under a little consideration. What are these points ? Why, in this case, and it appears it was the first, the only one of the kind that was met with, after several months' search at the Britain-street Hospital, the placental murmur was heard *faintly* across the fundus. This was the case that was to complete my ruin—the one the man at the mast-head was on the look out for for months before—the welcome announcement of breakers a-head, upon which what remained of my fragile bark was mercilessly to be drifted, and triumphantly dashed to pieces on. Well, the first thing here adopted is, that a case was got with the placenta *at* the fundus ; and it appears, the assistant physician of the hospital on duty, and the pupils, were called to witness the expected phenomena, or rather to bear testimony to the fact, that they did not manifest themselves, and, therefore, the triumph against me, and my

eternal disgrace. However, the first thing settled upon was, that a case was got with the placenta *at* the fundus. This I take leave, nevertheless, to be very sceptical upon ; for if the murmur was but *faintly* heard across the fundus, it must have come from the superior border of the mass, which I should only say was *near* the fundus. Now, I would ask, where was the lower border of it ? Why, at least, six inches below the upper, and perhaps more ; therefore, making one inch allowance for the placental murmur at the fundus being but *faintly* heard there, which is a very moderate allowance, the placenta must have extended seven inches, at the very least, downwards posteriorly ; in other words, I should call this not a case where the placenta was at the fundus, but one on the back of the womb completely. Thus then falls to the ground the first series of phenomena looked for—the first of the conclusives against me, namely, all those I stated in my paper *would* occur, if the placenta were situated *at* the fundus of the womb, without even taking into account the description of contractions on the supposition of which I spoke.

The next point against me, arising from this case, I strike under to completely and at once ; I certainly said the placenta was *never* heard at the fundus of the womb, and here unquestionably I am upset, for here is *one* instance of it ; I admit the justness of the charge.

The next against me, is one which is relied on as altogether *conclusive* on the subject. It is said, if my account of uterine contraction be true, it should happen that *as labour proceeded*, the murmur, heard first but faintly across the fundus, should have mounted upwards, be heard then in full intensity at the fundus, and after that have descended, till at length it almost arrived at the pubis ; whereas the observers, physicians, pupils, and all, *most explicitly* declare that no change whatever was observable in the position of the murmur, nor was there any alteration in its relative intensity in the regions of the uterus.—(p. 360.)

In all of these latter parts I most fully and cordially concur ; they are perfectly right ; nay, it could not be otherwise : and yet I can show they do not affect the question one particle.

In the first instance, I beg to state, I never said the placenta passed over the womb, nor could I say so ; I stopped it at the fundus, and even then, *after* the child was expelled, my words are : “ at length by this means the child is expelled, and *by that time*, the placenta arrives at the fundus.”—p. 480. But this apart, I shall proceed to show how impossible it was for what these gentlemen were gravely looking for, to occur in this or any case like it, and how irrelevant it is to our subject. In order to this, I have to state a matter connected generally with labour, which I am disposed to think they were not aware of at Britain-street at the time of making their observations upon this conclusive case ; or, if so, that the facts which I insisted on in my paper respecting the fixidity of the placenta to one spot on the womb, were not on their memory at the time ; and yet I am much surprised, as they were so intent watching every thing connected with this case, that what I am going to say did not then strike them.

The fundus, or uterine tumour, does not in labour descend, I think I may say, more than one inch and a-half, if so much, until the head of the child is passing the outlet. In the transit of the head from its resting on the brim, to its resting on the perineum, the general advance is not more than an inch and a-half, or thereabouts. Now, if we bear this in mind, and also what I have said in my paper, (p. 468,) “ on whatever part of the womb the placenta is first affixed, on that identical part does it remain, &c.,” we shall see how the case is altered as to supposing the bruit or murmur should have here mounted up to the top, passed then over it, and finally go down to the pubis, and which it was insisted on it should, if my theory were correct ; and further, how obvious it must be to any person, aware of this, that such a thing could not possibly occur, and, therefore, how its absence can in no way affect the question

before us. Until the head of the child is about being born, the relative position of the uterine parietes in fact remains nearly stationary; and how then could this change of posture as to the placental sound take place, the absence of which has been deemed at Britain-street so decisive against me, unless it were done by a shifting of the placenta itself upon the inner surface of the womb.

If the fundus descended to or below the umbilicus, during the stage of a labour I have just stated, namely, the transit of the head from the brim to the floor of the pelvis, then might this phenomenon be expected and looked for, because it is during that period that stethoscopic observation can be alone made; but it remains nearly stationary up to that period: it begins to descend as the head is about to escape; and when the head is born, what from the sufferings of the patient, the hurry and confusion of the moment, and the quickness with which the rest of the child is born afterwards, any thing like the use of the stethoscope is out of the question. I believe this is an answer, and a complete one, to this matter which has been thought so *conclusive* against me, and the looking for which occupied so much of attention in the wards of the Britain-street Hospital.

That what I have now said is a fact, every person can satisfy himself of by plain observation, and where such opportunities exist as there do at Britain-street, it can easily be ascertained; but if we consider what takes place in labour up to the time I mention, we shall see that such must be the case—the fact will be easily explained.

On the discharge of the waters the uterus contracts, and compresses the body of the child into the smallest possible space; the chin then sinks on the chest the head now begins to advance, but the most depending part of it then is the occiput; as the head advances, the chin, however, leaves the chest; and when the head is on the floor of the pelvis or perineum, the crown, which was in the slanting position of the brim when on the brim, now has attained a horizontal position, and the forehead is on the level of the occiput. Thus we see the depth

of the sacrum has nothing to say to the matter, in the way of measuring the general advance of the foetal body ; it becomes filled by the face of the child no doubt, but that is in consequence of the backward motion of the head during its advance, by the chin leaving the chest.* The general advance of the body, therefore, is only measured by the anterior depth of the pelvis, which is the depth of the symphysis pubis, or a little more. If now then we take into account the contractions of the womb, no matter how effected, we may readily perceive the diminution of it to propel the child, two, or say three inches, being chiefly in its (the womb's) superficial measurement, that the change in the absolute altitude of it will be less than the extent to which the foetal body is propelled ; that is, say one inch, or an inch and a-half at most. Whether this be a satisfactory explanation of the matter I shall not say ; but certainly the sinking of the fundus until the head is about to pass the outlet of the pelvis, is not even apparent. I believe I need say no more respecting this looking for an excursive movement of the placental murmur over the uterine surface ; it is as much in point as the supposing this to be a case with the placenta *at* the fundus ; the above fact respecting the descent of the fundus in labour, is more observable in first cases, where the tension of the abdominal parietes tends to keep the uterus compressed ; in subsequent ones, their relaxation admits of the advance of the fundus forwards,

* The final escape of the head through the soft parts, is very much accomplished by this backward motion of it, and without it, it would be difficult to suppose how rupture of the perineum could be avoided. If its escape were effected solely by the advance of the head, descending as it were perpendicularly, the perineum, which lies upon the crown like a broad band, must obviously be rent in most instances ; but combined with this throw back of the occiput, such a thing is not only thereby avoided, but a tendency thus given to the thin, expanded, band-like integument, to slip over the face in the direction of the coccyx, and so its integrity is preserved. Thus, then we see the chin is on the chest when the second stage of labour begins, and that from that, the head is impelled to its birth by a double or combined movement ; one, a general advance, and another, a tendency of the occiput to move backwards towards the spine, and which last is not at its maximum until the head is born.

so well known to every one, and which, as I endeavoured to explain in my original paper, resulted, or might result, from anterior uterine action.

The next conclusive point relied on, as arising from the sixth case, is, that as the membranes did not break until the head of the child was *deeply* in the pelvis, the reviewers think, on the supposition that all this time the placenta was revolving round the uterine tumour, and of course the membranes going round also, like planets in their orbits, that the orifice in them should have been different from what the first stethoscopic observation indicated, whereas it *positively* was not. This objection, however, is obviously based upon the opinion, that the fundus descended at the time I have shown it was nearly stationary; and therefore is neutralized by the fact I have related with respect to that opinion.

I would beg of the reader, after this explanation, now to go back to the conditions under which these cases are brought forward against me, and particularly as to the reviewers at Britain-street looking for placental detachment, from the anterior and lateral walls of the womb, with hæmorrhage, during labour. The phrase here “during labour,” we all know, is confined almost up to the birth of the head, the full delivery of the child being so quick after this, as scarcely to deserve notice in it; how then could detachment and hæmorrhage occur up to the resting of the head upon the perineum, if the superficies of the organ diminish in no perceptible degree? and after that, if detachment took place, it must be by permanent contraction; and how could hæmorrhage then occur, or how could any person, aware of the fact just stated, ever dream, much less speak of it? yet this is one of the questions gravely asked upon these cases. With respect to the objection that there is no *fixed point*, from whence contractions could have effect, such as I speak of, making the foetal body a fulcrum to produce their result; I do not think this is at all supported: in order to it, it should be first shown that there is a fixed point which will

prevent what are termed the longitudinal fibres from pulling up the mouth of the womb, and will give their action a tendency downwards, which, I believe, is not yet done : somehow, which we cannot explain, the lower part of the womb is preserved in a fixed state ; and this will apply to the one as well as the other theory. Indeed, with respect to uterine muscular fibres, whose existence are as yet, I believe, under dispute, I am disposed to lean to the negative side of the question ; I have in the several dissections in which I had an opportunity of investigating the matter, looked for these fibres, not with a view to their arrangement, but as to where such muscle existed as that to which we could refer the powerful action of labour, and which, I confess, I have not been able to trace ; and when I consider the excess of intensity with which this action exists, when the womb is under the influence of the ergot, I am disposed to think, that at least something else is in operation besides a few scattered fibres, such as are alone observable, to produce it.

We shall now proceed to consider the objection, as to the placenta and uterine surface not keeping pace together during their growth.

But before I examine the statements of our reviewers on this point against me, there is a matter connected with it which I really feel quite at a loss to reconcile, or indeed to understand.

I have complained of being *misconceived* ; and among other complaints from me on that score, that I have been reported to have said, “ if the placenta form its attachment *at any other situation*, (save the inferior part of the posterior wall,) *its growth cannot correspond to that of the womb*, and early separation and its consequences will follow.”—*Reply*, p. 346.

Now, as what *I have* said on this matter was, “ if the placenta formed its attachments *to the fundus*, and remained there during gestation, that these consequences would follow,” I should be disposed to attribute this incorrectness to mistake, were it not that in *the very page preceding it*, what *I really have said* is stated, and therefore would appear to have been

quite known when the next one was being written, where I have been misstated ; the words are, in the page preceding the misstatement :

“ Admitting the original position of the placenta at its formation to be *at the fundus*, Mr. Carmichael denies that it continues *in that relative situation* throughout gestation, and endeavours to prove the result of such arrangement should be separation, &c. ; and the reasons he assigns are twofold for these opinions. In the first instance, the principal growth of the uterus is, *as must be admitted*, at least with respect to a longitudinal direction, in that part of it, situated above the Fallopian tubes, *where* the enlargement, he says, is too extensive *to permit the placenta to keep pace with it, or maintain its connexion unbroken.*” —*Reply*, pp. 344, 345.

I would not for one moment suppose that the mistake was intentional ; it must have resulted from mistake alone ; but the cases that were “ *to sap the foundations of my fallacious theory,*” without it, had as much to say to the matter as they have to the circulation of the blood.

The reader, however, will see that in one place I am represented as quarrelling with the placenta being *at the fundus* ; and in the other, with its being *at any other situation* but the back low down. It is a hard matter for me or the placenta to keep our ground under such a cross fire.

I shall now, however, proceed to consider the arguments brought against me (pp. 351-2-3) to upset my assertion on this point, as to the two surfaces being unable to keep pace together *at the fundus*, for here I am *correctly* quoted on the matter ; “ let us now,” say our reviewers, “ follow up the process of gestation, and see whether there is any true ground for supposing the placenta cannot keep pace with the womb, if attached *even above the Fallopian tubes.*”

The first assertion on this point is certainly very novel to me, though given on the authority of Burns, and a reference made to morbid anatomy, namely, that the increase of the fun-

fundus is no more than five inches: the words of the reviewers are ; “the greatest development, in a longitudinal direction, takes place at the fundus, which increases from one-fourth of an inch, its measurement in the virgin state, (Burn’s Principles of Midwifery, p. 49,) *to about five inches*, as is evident *from the relative position of the Fallopian tubes.*”—p. 352.

Having assumed this then, a proportional calculation is gone into ; it is said the placenta is six inches in diameter (p. 352) at its full growth, and that at its first formation its diameter is one-half an inch ; thus then the size of the fundus and placenta at its full growth, and of the latter, at its formation, is given ; but as this last is in the *half*-inch measurement, the two others must be reduced to that measurement for the purpose of demonstrating their comparative sizes in these different states, and so we have 1 to represent the newly formed placenta ; 12 for it at its full growth ; and 10 for the fundus of the womb at its full growth : thus evidently showing the untenableness of my opinion ; and that so far from the fundus outstripping the placenta in its growth, the latter outstrips or encroaches on it in a proportion that would be represented by 2 ; therefore there can be no disturbance to it during gestation.

This certainly is all very good if the proportions given be correct, but there is one most unfortunate mistake in it, viz. *the size of the fundus* between the Fallopian tubes at the full period of gestation, which is the point at issue, and upon which all depends : let me be understood ; I mean the size across the fundus—five inches from one Fallopian tube to another *across* the fundus ? Why, its measurement in that direction is about eleven inches, which shakes most materially this fine calculation. In a marginal note, reference is made to Burn’s Principles of Midwifery, p. 49, for authority for this five-inch measurement ; I can only say that I have looked to the reference, and could not find it or anything about it.

In support of *my* statement, however, I shall beg to refer to the Museum of the College of Surgeons, where two prepara-

tions will be found of gravid uteri at full time ; and here it will be seen that I am correct in what I say, the fundal measurement, from one tube to another *across* the fundus, being fully eleven or twelve inches. We have a similar preparation at the Coombe, which bears also testimony to the truth of it. I was lately called to remove a child from a poor woman in the neighbourhood of the Coombe Hospital, who died about a week previous to her expected confinement, and the measurement carefully made on that occasion, from the surprise this statement from the Britain-street Hospital created amongst us there, was eleven inches. Now, if we make allowance for the contractions the womb undergoes after death, we shall see how very wide of the fact our reviewers are upon this matter. I do not, however, wish my statements here to be taken as admitted upon my own authority ; the museums I have alluded to are open to the public, and the preparations may be referred to and seen, to prove which of us is right, or which rather is wrong. I shall take the eleven inches, however, as the true measurement, in which, I believe, I am right ; and let us try this question by the proportions so corrected :—1 for the newly formed placenta ; 12 for it at its full growth ; 22 for the fundus of the womb at its full growth ; showing that the placenta does not increase during its growth upon the fundus, by a proportion which may be represented by 2 ; but that the fact is, the fundus increases on the placenta in a proportion that will be represented by no less a number than 10 ; so that if we take away the 2, contended for by my reviewers on the part of their placentas, and give it to the *masses* at the Coombe, the error in the calculation will be represented by 12, and that 12 will be in favour of my theory.

The next point against me is, “morbid adhesions of the placenta, slightly contracted pelvis, and even arm presentations do not vitiate the arrangements of nature ;” if they do not, I certainly say I do not know what will.

I have already given a reason, however, on my part, for con-

sidering that deviations of this kind *may* "vitate the arrangements of nature," and be "no authority, or standard, or rule in generation." I have done so in my publication of January last; and I shall again state what I then said. I have there urged the fact, that it is an observation made by many, that preternatural presentations occur most frequently where disfigurement of the bony structure exists in some shape; and from this I inferred, and, I think, naturally, that it seemed to require but a very immaterial departure from the healthy state of all parts, to derange the general order of things; and thence I went on to deduce, if deviation from the natural standard of the pelvis could so influence the *position* of the child as to *reverse* it, or throw it into a cross form so frequently, that we might suppose it capable of going farther and affecting the general harmony that should prevail as regards other matters, and so perhaps derange the position of the placenta, as well as those it did; and I shall show that our reviewers think so likewise, and have absolutely stated an example of it.

This is the train of argument, however, I used on that occasion, and to my moderate understanding it does not seem at all irrational; and I am happy to be supported in this opinion by this document coming from the Britain-street Lying-in Hospital, containing a statement to that effect. I shall go even further and say, that if I stood in need of proof of this my train of reasoning, these proofs are most satisfactorily supplied me by those very cases brought against me from the Britain-street Hospital. They have been five or six months observing the placentas at that hospital, with a view to my theory, and they have been enabled to bring forward but six or eight out of the entire that presented themselves during that period; and how are these six or eight circumstanced? They are cases of distorted pelves, gritty, diseased placentas, arm presentations, placental presentations, and the like. Why, this is the very thing I have been contending for, that it is in such cases aberrations of placental position do occur: instead of proofs against my reasoning upon

this matter, these very cases exemplify it. I cannot, however, but regret the mistake in not stating the situation of the hundreds of other placentas which were observed in Britain-street during the above period. If the position of these hundreds of other placentas had been stated, and also how the births went on in them, as to whether they were detached naturally, or by the hand introduced into the womb, and then compared these matters with those as observed in the five or six given, I think, at least, plain ordinary minds might be led to suppose that the testimony generally, very, very largely went in support of my doctrine ; and that the exceptions to it were to be found in gritty placentas, cross births, distorted pelves, placental presentations, *et hoc genus omne*, and therefore probably, that *I was* correct in saying, “any thing met with in such cases cannot be looked on as an authority, or standard, or general rule in generation.”

Our reviewers say, they cannot conceive “in what way an unusual contraction of the pelvis could modify the course the foetal vessels take to attach themselves to the maternal surface ;” and they protest most decidedly against such a doctrine. If they mean as to how it would explain such an occurrence, I cannot either ; neither can I or they explain how a contracted pelvis will modify the disposition of the child in the womb, so as to change its position from the natural one, yet this is a remark which, as I have already said, has been made by many. I think, however, I can show them absolutely adopting the very thing they protested so strongly against.

With all this declaration against a contracted pelvis, or bony disfigurement, possessing the power of influencing the place of the placenta, I beg to refer to their own fifth case, Elizabeth Hoey, one of disfigured pelvis, where the crotchet was used twice in consequence of this disfigurement ; and in this case the placenta was most irregular, so much so, that it was exhibited at the Pathological Society ; now the reviewers absolutely account for the irregular *position* of the placenta here, and how do they account for it ? Why, that it was in consequence of the

bony deformity ; the very thing they object to when I want to use it for that purpose. The words are, (p. 358,) “in this case the placenta had assumed a curious modification, in consequence perhaps of a deficiency of fluids, while the uterus *was thrown very much forward by the projection of the sacrum* ; the embryo had attached itself to the anterior wall :” what does all this mean ?

But the selection of cases are qualified by stating, “that it is only where some extraordinary difficulty exists, that we have an opportunity of satisfying ourselves on the point at issue by the introduction of the hand.” This I think, however, not the case ; I think an examination of the secundines, and an attentive observation of the phenomena that occurred during the birth, will give every information on it, as fully and more satisfactorily than testing natural cases by preternatural and anomalous ones, most of which these are.

I trust I have said sufficient in the foregoing pages to show the value of the arguments that have been brought against me in this review, and the extent to which my opinions are likely to be affected by them. The reader, I think, will also be satisfied, that the passages attributed to me in it are not exactly what are to be found in my original paper of January last, nor perhaps what would admit of being considered even obviously deducible from it ; and as upon these allegations alone, can the cases for one single moment be sustained, the force these latter have in the investigation, must, I should hope, be appreciated accordingly. I think also, the other arguments brought forward as expositions of my supposed fallaciousness, namely, the manner it is shown how the fundus in its growth can keep pace with that of the placenta ; the cases of diseased afterbirths ; of arm and placental presentations ; of distorted pelves, and such like ; now for the first time, I believe, put forward as specimens of natural cases, and intended confessedly to apply to natural pregnancies, together with the conclusive sixth case, and its destructive bearings on the matter : I say, I think, it must appear that all these rest

upon just the same stability, and affect my opinions just in the same degree, and no more.

I shall conclude, therefore, with the hope, that what I have written upon the placenta, &c. is not quite so ridiculous as this review would fain point it out to be ; and further, by observing that I shall not, on my part, be “deterred” at any time hereafter I may be induced to give expression to my opinions and observations, from protesting in the strongest possible language against any incorrect representation of what I shall write upon any subject I may be led to treat of, coming from where it may, or how it may. It is the right and duty of every person who feels himself so aggrieved to do so. I shall never take such a liberty with the writings of any person, neither shall any one or any institution do so with impunity with me, if I can help it ; and I therefore beg to observe, that the philippic lately published against me, and dated from the Lying-in (Britain-street) Hospital, for daring to exercise this right, and published chiefly for the purpose, as it confessedly declared in the commencement of it, “of deterring me and others from ever doing so again,” is altogether thrown away upon me ; I acknowledge no such authority in any institution.

It may be possible, what I have said respecting generation in these particulars is all erroneous ; if so, however, I should expect it to be met by a fair and candid canvass of the matter, which certainly has not here been done ; and if it should, and that it be so shown that I have been in error, I am ready to be the first to declare against it. But if, instead thereof, I am sought to be tried upon statements I never made use of, and so conclusions drawn against me calculated to hold me up to ridicule, I shall (in defiance of this “detering order,” lately pronounced and dated as above stated, and regardless of the “emblazoned shield” and the “high exaltations,” which an individual of so minor an importance as I am, has been sought to be dazzled with) set public opinion and judgment right on the matter, by pointing out, in no very measured language, the mis-

take in the premises from whence such conclusions are drawn ; and I am certain, in so doing, I shall experience, as I have in the instance gone by, the universal approbation and support of the Profession.

ART. XII.—*Contributions to Midwifery, No. II.—On Purulent Effusions into the Joints, &c. in Puerperal Women.*
By THOMAS EDWARD BEATTY, M.D., M.R.I.A., Lecturer on Midwifery in the Park-street School, and Consulting Accoucheur to the New Lying-in and City of Dublin Hospitals.

[Read before the Surgical Society, January 25th, 1839.]

IT having fallen to my lot to witness some examples of this rare form of disease in puerperal women, I am induced to make the following communication, more particularly, as although the affection has been treated of in detached papers by different authors, yet it appears not to have received the attention which its importance demands, as I cannot find it noticed in any of our most approved systematic works on midwifery. I am further induced to call attention to this formidable consequence of parturition, because of the obscurity in which it is sometimes veiled ; an obscurity which leads me to imagine that the disease may not be in reality so rare as it would appear to be, and that by a close attention to its nature, a greater number of instances might be discovered.

The affection to which I allude is purulent effusions in several and remote parts of the body, principally about the joints, and often communicating with their cavities. The symptoms which accompany the early stage usually commence within a few days after delivery, and consist of severe pain, tumefaction, and sometimes redness of one or more of the large joints, together with intense fever, loss of appetite and great thirst, and occasionally, but not always, pain and tenderness in the lower part of the abdomen. The absence of the last symptom leaves the case with

a strong resemblance to acute rheumatism, for which I have known it to be mistaken ; and this has led me to suppose that some cases described as the latter, and terminating fatally, in puerperal women, were instances of this disease ; and it thus accounts for the horror of rheumatism in lying-in patients which I have heard sometimes expressed.

Now there is no reason why rheumatism should be more fatal in a woman recently delivered, than under other circumstances ; but the disease under consideration is peculiarly formidable, and this, taken in conjunction with the resemblance between them at the commencement, serves to strengthen the above opinion. There is another reason why this disease may have escaped observation, even in cases in which it has proved fatal, and that is the very great obscurity of the local signs of purulent effusion which frequently prevails ; an obscurity often greater at the termination than in the earlier stages of the affection. This arises from the matter not being confined in any regular cavity, or circumscribed by any defined cyst, but being on the contrary effused into the cellular tissue, and extending wide under the integuments, and through the substance of the muscles. I have seen a case in which large collections of matter existed about the knee and shoulder joints, and communicated with their cavities, accompanied by extensive destruction of the cartilages covering the heads of the bones, in which all tumefaction and redness had subsided several days before death ; and the parts presented so little alteration from the usual external appearances, that a person who had not witnessed the course of the disease, would see nothing to induce him to make any particular examination.

From these circumstances it appears to me, that the addition of a few cases more, to those already recorded, may not be an unacceptable nor unprofitable contribution.

The first instance of this affection which came under my notice was in the case of Mrs. B., a young lady aged 20 years, whom I saw, in conjunction with Dr. Shekelton, after her first

accouchement, in December, 1831 ; and during the progress of the disease we were assisted by Dr. Graves and Mr. M. Collis.

This lady was seized immediately after delivery with severe uterine hæmorrhage, for which she was treated by Dr. Shekelton in the usual manner, and with success. Among the other remedies employed on that occasion, the free application of cold water to the abdomen and nates was had recourse to. The hæmorrhage being arrested, all matters appeared to be going on favourably for some days, when febrile symptoms, preceded by rigor, made their appearance, accompanied by severe pains in different parts of the body, particularly low down in the back, and in the neighbourhood of the large joints.

The sufferings of this patient were most intense from the the first invasion of the pains to her death, which did not take place until the expiration of six weeks. The slightest attempt at motion produced such intense agony, that she lay permanently on her back ; and although severe pain was complained of in that region, it was utterly impossible to make any examination into its condition. The hips, knees, and shoulders, appeared to be the joints most affected ; and although some redness and tumefaction were present in those situations, there was little that could lead to the supposition that purulent matter was deposited, except towards the close of the case, when a large, doughy, uncircumscribed swelling was observed on the inner and upper part of the left thigh, into which an opening was made, and a quantity of healthy pus was discharged. The pulse from the period of the rigor was seldom below 120, often more frequent, and soon became small and feeble. The thirst was considerable ; and the tongue was dry, red, and glazed. The countenance was flushed in the commencement, and soon became sunken, contracted, and muddy ; and the face, together with the rest of the body, was frequently bathed in perspiration. Whilst lying immoveable in this state of suffering, it was found about the third week of her confinement, that the under sheet of the bed was soiled by matter, which was at first supposed to

proceed from a bed sore. It was then determined that an examination should be made ; and, accordingly, two strong persons undertook to raise her in the horizontal position from the bed. This being done, while she was suspended in the arms of the assistants, the back was examined, and it was found that a portion of the integuments, of the size of a crown piece, had given way over the upper part of the sacrum, from which opening purulent matter dropped freely. The edges of this opening were unattached to the parts beneath ; and on making pressure in the neighbourhood, a large quantity of pus was expelled ; but there was no hardness of the surrounding parts, indicative of a limitation of the purulent deposit. No abatement of the constitutional symptoms followed the opening just described, but on the contrary, the condition of the patient became daily more and more distressing. The irritative fever increased ; the pain on motion continued excessive ; and the daily repetition of elevating her from the bed, in order to apply fresh dressing to the back, was a scene of agony to the sufferer, and to those in attendance upon her. In this state she lingered until death put a period to her sufferings. We were permitted to make only a superficial examination of the body, and found an extensive detachment of the skin of the back in the situation mentioned. The hand could be passed in all directions under it, and there was no defined limit to the separation. I regret we could not make any more particular inquiry, as I have no doubt that we would have found purulent deposits in most, if not in all the parts in which pain had been complained of.

The second case was that of a woman named M'Evoy, aged 28 years, who was delivered in the Lying-in Hospital, Cumberland-street, after a short and favourable labour, on the 28th of December, 1836. In the course of the subsequent night she was seized with a severe rigor, which lasted for a considerable time, and on the following day she was found affected with severe pain in the large joints, particularly the knees, shoulders,

and elbows. The pains were so excruciating as to cause her to scream with agony even when at rest, and to deprive her totally of the power of voluntary motion. Her abdomen was not at this time unusually tender, nor did she experience any tenderness in the uterine region for some days after the accession of those symptoms. The pulse was 120, and hard, and the countenance indicated intense suffering. The thigh on the left side began to swell, but not to any great degree, nor did it present any redness of the surface. Venesection was employed on the first attack of the disease; and the use of calomel and opium was adopted without delay, in the hope of bringing the system under its influence. The calomel, however, soon produced diarrhoea, and passed away by the bowels. At this time the hospital was visited by puerperal fever, and as I determined to close the wards for the purpose of purification, this patient was removed to the City of Dublin Hospital, on the 2nd of January, 1837, and placed under the care of Dr. M'Adam, with whom I continued to see her until her death.

On her admission into the latter hospital, she was perfectly helpless, and it was with great difficulty she was carried to her bed. Her countenance at this time was highly flushed, and expressive of great agony; her respiration was hurried; her face suffused with perspiration. The pulse full, quick, and bounding; tongue dry and red. Voluntary motion was impossible, and even passive motion could not be effected without great torture. The joints most complained of, were, the left knee and elbow, and the right shoulder. The affected joints were red and swollen: the redness was very bright over the right shoulder. Diffuse patches of redness were also visible on the left forearm, and back of the left hand, and middle finger, and also on the calf of the right leg. She complained of considerable pain in the left thigh, as well as in the uterine region of the abdomen. The diarrhoea still remained, and her thirst was insatiable.

January 1st. Spent a sleepless night; the pains still severe

and unremitting; abdomen very tender, and rather tumid. Pulse not so full as yesterday; tongue red, dry, and glazed. Leeches were applied to the abdomen, and calomel, gr. ii. and opium gr. $\frac{1}{2}$, ordered every second hour.

4th. Total absence of sleep; complains more of pain in the left thigh, which is perfectly powerless. The pain in the right knee and elbow not so severe, and she has some command over these joints. Pulse smaller, 130; tongue red and dry; some appearance of sordes on the teeth; bowels now rather confined. On examination the left thigh was found considerably swollen, perfectly pale, tense, hot, and extremely tender, in short labouring under phlegmasia dolens. The tenderness is greatest in the groin, and towards the outer part. Leeches were applied to the groin, calomel and opium to be continued.

5th. Yesterday evening the diarrhœa returned, accompanied with severe tormina. Her pulse became very intermittent; the respiration hurried and difficult, together with severe pain in the præcordial region. The calomel was now omitted, and mercurial inunction, together with Hyd. c. Creta, and Dover's powder, were ordered. A blister was applied over the heart; this had the effect of relieving the pain and difficulty of breathing.

6th. Pulse still intermitting; pain and tension of left thigh increased. Abdomen very tender and tympanitic; countenance occasionally highly flushed; tongue dry and glazed. Leeches were again applied to the abdomen: she was ordered camphor mixture, with carb. ammon. The mercurial frictions were continued; and beef tea *ad libitum*.

8th. Pulse though strong still intermits. The tympanitis has subsided, but considerable tenderness of the abdomen remains. Left thigh as before; great pain in the left knee and elbow; right knee and elbow much better. Numerous aphthæ were discovered on the inside of the lips and fauces. Leeches again applied to the abdomen, followed by fomentations.

9th. The intermission in the pulse has now ceased, but

she is harassed with frequent attacks of violent diarrhoea, which were checked by the administration of pills containing a grain of opium each, and by anodyne enemata. The pain and tension of the thigh have decreased, and she can now move it slightly; the abdomen also is less tender. Ordered a repetition of the leeches and mixture.

10th. The patient appears on the whole better; her tongue is now moist, but the aphthæ still remain.

Reptr. hirud. et mistura.

11th. Diarrhoea still urgent, unless when controlled by opium. The local affections are considerably improved; a small abscess on the back of the middle finger of the left hand was opened. A red patch was this day detected on the back part of the left thigh. Her pulse is smaller, 130; the tongue again dry, red, and glazed. The countenance is more anxious and collapsed; the respiration is hurried. The stimuli were increased; and she was ordered to have mulled wine and opiates as before.

From this date the constitutional symptoms increased in severity, the diarrhoea continued, she became more and more weak, and finally died exhausted on the 23rd; just twenty-six days from the time of delivery.

On examining the body our attention was first directed to the abdomen and uterus; and here no morbid appearance of any serious import was discovered. The peritoneum was free from disease, and a trifling quantity of serous fluid existed in its cavity. The uterus was to all external appearance healthy, and no result of inflammation could be detected on its surface; but on cutting into it, the veins were found thickened, and at some distance from the organ were filled with dark coagula. The hypogastric and iliac veins were also inflamed, thickened, and lined with the same material. All appearance of tumefaction and redness had so completely subsided from the joints affected by pain, some days previous to death, that we did not expect to meet much morbid affection in those situations. To our sur-

prise, therefore, on making an incision through the integuments, on the outside of the left knee, a large quantity of healthy pus flowed out. This was effused under the integuments, and in the cellular tissue of the neighbouring parts, and extended into the cavity of the joint, the cartilage covering the heads of the bones being very much eroded in many parts. A similar exploration was made of the right shoulder and elbow; and in both situations large deposits of purulent matter were found; that about the elbow communicated with the joint, in which there was extensive destruction of the cartilages. That in the neighbourhood of the shoulder was extensively diffused amongst the muscles, but did not open into the articulation.

The third case to which I will allude, was a highly interesting one, the particulars of which were stated at the Pathological Society by Professor Harrison,* and the recently removed parts, together with a drawing, were exhibited by him on that occasion.

Anne Grumley, æt. 30, was delivered of her second child in the Lying-in Hospital, Rutland-square, on the 9th of March, 1839, after an easy and favourable labour. Five days after delivery she was attacked with fever, and symptoms of inflammation of the uterus, from which she was relieved by appropriate treatment, and was discharged from the hospital on the 24th of March. A few days afterwards, finding herself quite well, she returned to her situation as cook in a gentleman's family; but she soon perceived an erysipelatous swelling of the right ankle, to which she applied hot turpentine, which considerably aggravated the symptoms; and she was removed to Sir Patrick Dun's Hospital on the 2nd of April.

On admission, the right ankle and dorsum of the foot were swelled, hot, painful, and of a bright red colour; the leg was also swelled, but colourless. These symptoms were accompanied by a low fever.

* Dublin Medical Journal, vol. xv. p. 510.

7th. The limb generally was swelled and hot ; she was attacked with purging last night.

11th. The swelling has been gradually increasing, and engages the whole limb up to the groin. It is colourless, shining, soft, but not pitting on pressure, not attended with pain on pressure except in the groin, and along the inside of the thigh. Motion of the limb gives great pain. The patient is very weak, and lies constantly on the back. Delirium at night ; abdomen tympanitic ; purging continues : first sound of the heart almost inaudible.

13th. Pulse 130, very weak and indistinct. Delirium at night. Tenderness in groin less ; glands in groin slightly enlarged. Complains of acute pain in the back, and inside of the *right* knee on the slightest motion, but not much on pressure ; no pain on flexion of the ankle joint. She is very irritable and restless, constantly moaning.

15th. Pulse 130, exceedingly weak. Pain gone, except on moving the leg ; swelling shining, hot, not tense, pitting on deep pressure ; countenance sunk ; restless. Slept last night without an anodyne.

16th. Pulse 135, a shade stronger. Had a quiet night ; three dejections since yesterday ; was very much excited in the evening. A blue spot has appeared on the sacrum ; limb same as yesterday.

17th. Pulse 132, very feeble and indistinct ; skin very hot ; subsultus tendinum ; tongue dry and brown ; countenance very much sunken, has the appearance of a corpse ; she lies motionless on the back. A gangrenous spot on the right heel ; raved a little during the night ; only one motion since yesterday ; refuses nourishment.

18th. Looks a little better. Pulse 140, very weak. Delirium this morning ; spot on the back enlarging ; gangrenous spots on both heels ; shining appearance of the skin of leg gone ; feeling of fluctuation in the right knee.

19th. Swelling of limb subsiding rapidly. She died this

evening at six o'clock, just six weeks from the time of delivery.

On examination after death, all the venous branches of the thigh, leg, and foot, with the iliac vein, as high as the inferior cava, were filled with grumous blood, mixed with a substance like purulent matter; the lining membrane of the veins was rough, and had lost its natural polish; the spermatic veins were thickened as they approached the vena cava; and the uterine veins were remarkably indurated. The internal iliac, uterine, and spermatic veins of the right side were similarly affected. There was a deposition of pus in the ankle joint, and also in the knee joint; the cartilage of the patella was abraded; there was also a slight purulent deposit in the body of the gastrocnemius muscle; this abscess communicated with one of the veins; the lymphatic vessels were healthy; the integuments and subcutaneous cellular tissue were hard, and filled with serum.

Having been so fortunate as to see this case, I was struck with the resemblance the veins, when separated from the body, bore to the plates illustrating Dr. Lee's paper on Inflammation of the Veins of the Uterus, &c. in the *Medico Chirurgical Transactions*, Vol. xv. Part 1, to which plates I would refer the reader who wishes to have a more accurate idea of the condition of the veins in Professor Harrison's case.

This case is peculiarly important, as it shows the connexion between the inflamed vein and a purulent deposit, more clearly than any other with which I am acquainted.

Mr. Palmer informs me, that in June, 1837, a young woman was admitted into Mercer's Hospital, labouring under phlegmasia dolens, with which she was attacked in about ten days after delivery. She died in eight weeks after admission; and purulent matter was discovered in all the large joints of the extremities, in several of which the cartilages were extensively eroded.

There are few tissues in which purulent deposits have not been found as well as in the joints. They form in the brain, in

the eye, in the cavities of the thorax and abdomen, in the muscular tissue of the heart, in the parenchyma of the lungs, liver, and spleen, in the substance of the uterus, in the ovaria, and in the external cellular membrane. It is the opinion of Andral and other pathologists, that these purulent deposits are the result of absorption of pus; and that the pus after it is absorbed from the suppurating cavity, is separated from the blood on the surface or in the interior of the various organs.

But this explanation is disproved by the second case just mentioned, for in it the formation of pus in the distant localities cannot be attributed to the absorption and transference of that fluid from one situation to another; no matter having been found in the uterus, from which organ it has been supposed to be carried in other cases. There was hence no origin or source from which the purulent matter could be derived; and we are forced to conclude, that its appearance in the distant and separate places in which it was discovered, was the result of an inflammation of a peculiar kind. What this inflammation is, has, I think, been very clearly demonstrated by Mr. Arnott in his highly valuable paper on the secondary effects of inflammation of the veins.* It is impossible to compare the great number of cases of phlebitis quoted by Mr. Arnott, and arising from a variety of causes, accidents, operations, &c., with those just related, and not be struck with the strong resemblance existing between them. In some of Mr. Arnott's cases, injury of a vein, as by ligature, was the exciting cause of the subsequent fatal malady. And in others, succeeding to amputation, the venous inflammation was distinctly traced to the surface of the stump. In like manner the source of fatal cases following parturition, has been found in the uterine, hypogastric, and spermatic veins, and thence extending to the vena cava, by Mr. Wilson,† M. Louis,‡ Dr.

* Med. Chirurg. Trans. vol. xv.

† Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, vol. iii.

‡ Arch. Générales, Mars, 1826.

Davis,* M. Velpeau,† Dr. A. Lee,‡ Cruveilhier, and in the case by Mr. Harrison, not only were the uterine and neighbouring veins found inflamed, but the inflammation had extended to the iliac veins, and thence upwards to the cava, and downwards to the small branches of the leg, in the calf of which one of the purulent deposits was found, actually communicating with the cavity of a small vein.

It has been most satisfactorily shown by Dr. R. Lee, in his researches respecting the pathology of “*Phlegmasia Dolens*,” that this affection owes its origin to inflammation of the uterine veins, thence extending to the iliac and femoral veins. Now it is worthy of remark, that in the four cases I have alluded to, as well as in many similar on record,§ this peculiar affection was present, and formed a prominent feature in the course of the disease. Seeing then that these purulent deposits in the joints and elsewhere, are frequently produced in cases not puerperal, by direct injury of a vein, and the inflammation consequent thereon; and observing also, that when these effusions of pus take place in the puerperal state, they are so constantly accompanied by an affection known to be produced by venous inflammation, it is, I think, fair to infer, that the formidable disease under consideration is to be attributed to the same cause. This view of the subject would lead to the conclusion, that there is an affinity between the two affections; and I am strongly disposed

* Med. Chir. Trans. vol. xii.

† Arch. Gen. October, 1824.

‡ Important Diseases of Women.

§ Two cases related by M. Velpeau, in his paper on *phlegmasia alba dolens*, bear strongly on this point. In one of these death took place on the sixtieth day after labour; in the other, on the twenty-sixth, under great constitutional disturbance and exhaustion. In both, purulent matter was contained in the hypogastric and femoral veins. In the first case, the interpubic cartilage was softened, and pus was found in this situation; the same appearances were found in the left sacro-iliac symphysis; the hip joint also contained purulent matter. In the second instance, the sacro-iliac and pubic symphyses were in a state similar to that just mentioned.

to think that they are essentially similar, but differing in degree of intensity. The cause of the inflammation is in both cases to be found in the sudden and great changes effected in the condition of the uterus by delivery. In fact, after the accomplishment of the process, this organ may be regarded as an injured part. The violent and often long continued efforts made by its muscular fibres to expel the foetus, whereby, after the discharge of the liquor amnii, the inner surface of the cavity is brought into apposition with the body of the child, and strongly compressed against it; the sudden separation of the placenta, which had been so long in close connexion with a large portion of the uterine surface; the sudden exposure of the large orifices of the uterine blood-vessels, which had been hitherto sealed by the presence of this organ; the probable admission of atmospheric air into the cavity, in consequence of the imperfect contraction, or subsequent relaxation of the uterus. All these acting on an organ so highly organized as the uterus is at the full period of gestation, must be regarded as so much violence done to it; and when to this we add the presence of the hand in its cavity so often required in turning, and in the extraction of a retained placenta, and further, the free application of cold to assist in restraining hæmorrhage, it becomes a matter of wonder, not that this organ should be often excited to inflammation, but that it so frequently escapes.

The immunity of some patients from serious consequences after the most protracted and difficult labours, and the apparent readiness with which others fall victims, in whom delivery has been easily accomplished, lead us to look for some remote or predisposing cause to account for such results. This, I think, will be found on inquiry, divisible into two kinds; the first existing in the patient herself; the second in external influences. With respect to the first, it would appear that there is in some persons a peculiar aptitude or disposition to run into inflammation from trifling causes. If we look into the history of phlebitis in general, we will find that this is manifest, no matter what

the immediate cause may be. How many persons, for instance, suffer the operation of venesection with impunity, yet now and then rapid death from phlebitis is the consequence. This result cannot be attributed to the simple operation, from which so many suffer no ill effects, but clearly to some peculiar habit in the patient. In like manner, after amputations, numberless cases go on to perfect recovery, while in some rare instances, venous inflammation sets in, extends rapidly to remote situations, and destroys the patient. Again, in that formidable disease which sometimes follows wounds received in dissecting, (a disease which if not identical with, has at least a great resemblance to phlebitis,) how rarely do we see it occur in comparison with the number of wounds received every session; and how often do we find one individual suffer repeated attacks, while his fellows escape free. This can be accounted for, only on the supposition that there has been some peculiar susceptibility in the few unfortunate sufferers; and I believe it will be found on inquiry, that in the majority of these cases the individuals were in a bad state of health at the time, produced by over exertion of mind or body, or perhaps both. I have been informed by Dr. Houston, that the late lamented Mr. Shekleton was broken down in health by over fatigue in dissecting a celebrated horse of Sir Colquhoun Grant's, when he received the fatal wound which deprived the profession of one of its brightest ornaments. And in the case of my esteemed friend, and former pupil, Dr. Mayne, of the Richmond School, who has just recovered from a severe attack of this disease, he was on this, as on a former occasion in which he suffered, in bad condition from over work in the discharge of his duties, in which he is so notorious for energy and zeal. The second kind of predisposing cause consists in exposure to noxious external influences. In the second case related above, it will be observed that the patient was seized with phlebitis while puerperal fever was present in the hospital. On the occasion when this paper was read before the Surgical Society, Mr. Cusack said "he had observed the disease just

described in females after parturition, and thought that it varied according to the epidemic constitution of the period, and other similar influences. He thought that when inflammation of the periosteum, or synovial membrane, took place after childbirth, it proved healthy or unhealthy according to the state of the constitution, season of the year, epidemic tendency, &c. independently of parturition."

In like manner, Dr. Murphy said, "that he had also seen cases of the kind described by Dr. Beatty, and had noticed that they preceded the invasion of puerperal fever, indicating as it were the occurrence of a change in the atmospheric constitution, preparatory to the approach of that disease."

With the nature of this malignant influence we are entirely unacquainted, but we are too familiar with its effects in the occasional prevalence of erysipelas, and puerperal fever conjointly; and to its presence may, I think, be ascribed the occurrence of some of the cases of phlebitis that present themselves.

The disease under consideration is one of a very fatal character. All the cases of it that have come within my own knowledge have terminated in the death of the patient. A similar result took place in the instances mentioned by Mr. Arnott, by Dr. Marshall Hall, and Mr. Higgenbotham, and by Dr. Lee; and it is stated by Mr. Arnott, that "on inquiring of Dr. Meriman concerning a disease of the joints in puerperal women, that gentleman informed me that he had seen such cases, but that he did not know of one that had terminated favourably." Mr. Cusack, however, informed me that he had seen some cases recover, with ankylosis of the affected joints. The symptoms so soon assume a typhoid character, that general bloodletting is inadmissible, except in the very commencement; and although local depletion by leeches gives a temporary relief, it does not appear to check the progress of the disease. Mercury, which so powerfully controls inflammation in general, seems to have little influence in this disorder; and I have remarked a peculiar resistance to its specific operation in patients afflicted with

phlebitis. It is almost impossible to produce salivation, no matter in what way the mineral is employed, and if given by the mouth, it is very frequently productive of distressing diarrhœa. In fact, until we arrive at the knowledge of a more successful way of combating phlebitis in general, I fear that midwifery practitioners will have to bear the same testimony to the fatal nature of these cases. I would be strongly disposed to try the effects of turpentine or the balsams, in any case that may unfortunately occur in future, from the known effects of these medicines in deep seated inflammations of the eye which do not yield to mercury. The observations of Mr. H. Carmichael have shown, that turpentine is capable of arresting iritis in cases where mercury has failed; and Mr. Rynd has informed me, that he has lately employed the balsam of copaiva with great success in similar instances.

In reflecting on the fatal nature of this form of puerperal phlebitis, the question naturally occurs, why it is, that although arising, as has been stated above, from the same cause, and being of the same nature, phlegmasia dolens should be nevertheless a comparatively tractable and innocent affection? The answer is, that although of the same origin and kind, the two affections differ in degree of intensity. This is consonant with what we know of phlebitis commencing in other situations, and from different causes. In some, the disease is limited to the veins in the neighbourhood of which it originates, and the patients recover; in others, the venous inflammation is more extensive, it attacks distant parts, and after a struggle of some weeks destroys its victim; in others again, the case is more rapid, and runs its fatal course in as many days. We have the testimony of Mr. Freer, quoted by Mr. Hodgson, that dangerous symptoms came on suddenly, four hours after ligature of the saphena, and soon terminated the life of the patient.

In like manner, we find different degrees of phlebitis arising from inflammation of the veins of the uterus after parturition. 1st, the milder form producing the phlegmasia dolens; 2nd, the

more severe, producing, in addition to this affection, purulent effusions into the joints and elsewhere, and running a course of from four to eight weeks: and to these I would be inclined to add a third degree, parallel to the case mentioned by Freer, a degree in which death takes place in a very few days. Mr. Travers* draws a distinction between the cases where the inflammation of a vein terminates in the formation of pus, and where it terminates in the deposition of adhesive matter, or lymph. He observes there is a marked difference in the symptoms accompanying these states; the first is a protracted irritation producing hectic, and ending in exhaustion; the second is a typhoid fever, which speedily producing delirium, terminates within a few days. Under this last head I would include some of those awfully rapid cases of puerperal fever, in which death takes place in twenty-four or forty-eight hours after delivery, and in which but trifling *post mortem* appearances are to be found.

ART. XIII.—*Observations on the Treatment of Acute Rheumatism by Opium.* By D. J. CORRIGAN, M.D., Physician to Jervis-street Hospital and to Cork-street Fever Hospital, Lecturer on the Practice of Medicine at the Dublin School of Anatomy, &c. &c.

“As to the cure of rheumatism I have often been troubled that it could not be performed without the loss of a great deal of blood, by which the patient is not only much weakened for a time, but if of a weakly constitution he is most generally rendered more obnoxious to other diseases for some years, &c. &c.”—SYDENHAM.

THE exquisitely practical writer from whom I have taken the above quotation is held up by the advocates of the rigid anti-phlogistic treatment of rheumatism as their prime authority; but while they adduce those passages† which coincide with

* Cooper and Travers' Surgical Essays, vol. i.

† “I think the cure ought to be managed by bleeding. As soon as I am called I order ten ounces of blood to be taken from the arm; the next day I order the

their own views, they generally omit noticing the modifications* which Sydenham's opinions afterwards underwent. Sydenham's rules as to the treatment of acute rheumatism by bleeding, (and which indeed are very analogous to the more modern *coup sur coup* plan of Bouillaud,) were afterwards, as evinced by the passages quoted in the notes, very considerably modified by his subsequent experience, and would perhaps have been still more altered, but for his hypothesis of the nature of the disease.

From Sydenham's time up to the present, there have been varying opinions on the treatment of acute rheumatism, (such as appear to have existed at different times in Sydenham's own mind,) some practitioners (the majority) adhering to bleeding and antiphlogistic measures; while others, as Haygarth, have recommended remedies of so opposite an action, as bark. The existence of these varying opinions may prove that it is possible to cure the disease by different or opposite lines of treatment; but still it will be readily granted, that there may be some one of those varying plans of treatment which must be superior to the others, either in shortening the duration of the disease, in preventing the occurrence of those very serious complications, such as pericarditis, endocarditis, or metastasis to internal organs, which occur in rheumatism, or (what is of the greatest consequence to the class most subject to rheumatism) in husbanding the patient's strength, the loss of which would otherwise leave him, as Sydenham observes, not only much weakened for a time, but rendered more obnoxious to other dis-

same quantity of blood to be taken away; and a day or two after, according to the strength of the patient, I bleed again; and then after three or four days, I bleed the fourth time, &c."

* Referring in an after chapter of his book to the passage quoted in the preceding note, he says: "since I wrote that, I have found by experience, it is better after the second, or at most after the third bleeding, to give cathartics, than to do all by bleeding; the cure may be thus carried on without a great loss of blood, &c." The general tenor of the accompanying passages is laudatory of treatment in which bleeding had been omitted.

eases for years to come. The treatment by opium, I believe, will be found to possess the advantages sought for. It shortens the duration of the disease; it enables the patient to pass through so painful a disease with comparatively little suffering; it husbands his strength, so that when convalescent he rises from bed with comparatively little depression of vascular energy and muscular strength; and lastly, it diminishes very remarkably the tendency to the occurrence of such complications as pericarditis, endocarditis, &c. Before proceeding to contrast the merits of this plan of treatment with others of an opposite kind, I shall relate some cases treated on the plan here advocated; they will also show the rules as to quantity, time, &c. which have been observed in administering the opium. It will be observed, that in the three first cases calomel was combined with the opium. I feared at first, indeed, to venture on the opium alone in large and frequently continued doses, but as my experience increased, I trusted to the opium alone.

CASE I.—Mary Anne Mullen, ætat. 17, was admitted into Jervis-street Hospital, on the 8th March; she had been ill seven days. She was unable even to turn in the bed. Her wrists, elbows, knees, and ankles, were tense, red, swollen, and acutely painful; her shoulders and the back of her neck were also very painful and stiff. From the severity of the pains, in the red and swollen joints, she lay in the bed utterly helpless. Her skin was hot; her pulse 108, soft and full; her face was covered with perspiration; her tongue moist and yellow; her bowels were confined. She got that day ten grains of calomel, and four grains of opium, in four pills, one every fourth hour, and a purging mixture of colchicum the next morning. On the 9th, 10th, and 11th, the pills were continued; and on the night of the 11th she had but little sleep. On the 12th inst., the opium was increased to two grains every fourth hour, while the calomel was diminished. She took eight grains of opium within the twenty-four hours, and the quantity produced no effect on the

head. On the 13th inst. after a castor oil draught, she took a grain and a half of acetate of morphia, at bed-time; on the 14th inst. the same quantity. On the 16th inst. she was put on quinine; and on the next day, 17th inst., she was reported quite well.

CASE II.—Con. Egan, ætat. 19, was admitted into hospital January 15th, 1838. About three weeks previously, his illness began after exposure to cold, first with pains in his knees, ankles, and feet, which afterwards extended to his shoulders.

On admission his tongue was furred; his pulse was 112; and his skin was hot. His shoulders were so painful that he could not move his arms; and his wrists and finger joints were swollen, red, and painful. He complained of tenderness on pressure over the region of the heart, but there was no morbid sound. He got six pills, each containing one grain of opium, and two of calomel; these he took within the twenty-four hours. On the next day, the 17th inst., he took the same quantity of opium and calomel; on the 18th the same quantity; and on the 20th the opium was administered without the calomel. On the 25th inst. the opium was increased to nine grains within the twenty-four hours, in doses of a grain and a half every fourth hour. On the 29th, he was put on quinine; and on the 30th, he was reported perfectly well.

CASE III.—On January 19, 1838, I saw under the care of my friend Mr. Doyle, Mr. R., æt. 30. Having for several days previously felt flying pains, he has for the last three days been suffering under most acute pain in the shoulders, in the back of the neck, along the loins, and in the knees, wrists, and ankle joints. The knees, ankle, and wrists joints, more particularly, are swollen, red, and exquisitely painful. He has slept none for three nights; pulse 132; bowels free; tongue white, thickly coated; urine remarkably high coloured, depositing a pink sediment; skin partially perspiring. One grain of opium with two of calomel was given every third hour, and opiate fomentations were applied to the joints. On the next day the pains

were less, and the pulse had fallen to 120, but there was no sleep. The opium was increased to a grain and a half every third hour.

On the 21st there had been some sleep; the pulse had fallen to 104; the pains were greatly diminished, and the swellings of the joints equally lessened; the opium was continued as on the previous day. The pulse next day, 21st, had fallen to 92, there was good sleep; the opium without any calomel was continued in grain and a half doses every third hour for three days longer, when on the 24th, being free from pain and with a quiet pulse, he was put on bark.

CASE IV.—Michael Quinn, æt. 33, was admitted into hospital on the 16th December. About fourteen days before, after a severe wetting, he was attacked with pains in the ankles, which on the next day spread to the knees, hips, and back. On the day of admission the left ankle, left shoulder, and right knee were painful, the knee and ankle were hot and swollen; his bowels were free, his pulse was 104, his tongue white and coated. A warm fomentation of decoct. chamæmel. c. sp. camphorat. was ordered to be applied to the affected joints, and he was directed to take a pill of one grain of opium with half a grain of sulph. quinin. every fourth hour. He took four of these pills; on the next day he took, every third hour, half a grain of acetate of morphia, until he had taken, in all, three grains of it.

On the 18th the report was that he had good sleep, that the pains were very much diminished; the skin was moist, the pulse 104. The same treatment was continued, but with an increase in the quantity of acetate of morphia; the dose being now increased to four grains of the acetate within the twenty-four hours, half a grain being given every second hour. On the 19th the same treatment was continued; on the 20th the report was, that the wrists and elbows were free from pains and stiffness, and the pains in shoulders very slight, but both knees were painful; he had good sleep, the pulse was 104. The acetate of morphia was continued; on the 21st and 22nd

the same plan of treatment was persevered in ; and on the 23rd, the report was :—

“Pulse soft ; skin cool ; sleep good ; tongue moist ; very slight pain in right knee, and only felt on moving it : no pain in any other joint ; bowels free.” The acetate of morphia, with the addition of two grains of camphor to each pill, was continued ; and he was put on beef tea. On the 26th his bowels were *too free*, and he got chalk mixture with *tra. opii* ; and the diarrhoea being arrested, he was ordered, in his convalescence, a grain of quinine with two table-spoonfuls of *mist. guaiaci* three times a day.

CASE V.—Mr. C. K ———, ætat. 20, had felt unwell for four days, from chilliness and loss of appetite. On the fourth day, October 27, his wrist, elbow, and shoulder, began to swell and to give him pain. On the next day, the 28th, the joints were much more swollen and painful ; his pulse was jerking, and rose to 120. The tongue was very white ; and the urine was deep red. He got one grain of opium every third hour. Next day, 29th, he was easier, and had passed a good night. The same plan of treatment was pursued on the 30th, and he continued to improve ; but on the 31st the rheumatism became flying ; it quitted the wrist, elbow, and shoulder, of one side, but attacked in return the hip and knee of the opposite side ; his pulse had, however, fallen to 92 ; the opium was now given in increased doses, *gr. ii.* every three hours. For the next two days the rheumatism presented the same flying characters, one hour very severe in the joints of the lower limb of one side, then subsiding, and attacking with equal violence the joints of the upper extremity of the opposite side. In some of those flying attacks his pulse became weak ; he felt oppression and constriction across the chest ; his countenance became very anxious ; and he was bathed in profuse perspiration. One grain of opium, and $1\frac{1}{2}$ grains of sulph. quinine were now given every third hour. On the 3rd November the improvement was most marked ; the pains were nearly quite gone ; the pulse was 84, and steady. The medicine was

continued as precautionary treatment ; and on the 9th he set off for the country. In this case the symptoms were for two days very urgent, and at several times the disease seemed on the very verge of running into pericarditis ; but by perseverance in the plan of treatment adopted, the patient, within twelve days of the onset of the rheumatism, was perfectly free from complaint, and was able, with very little diminished strength, to undertake a journey to the country.

CASE VI.—On Wednesday, 24th April, I visited my friend Dr. Aldridge, in a very severe attack of the disease. Nearly all the large joints were swollen and acutely painful, and the pains were shifting from joint to joint. The pulse was 120 ; the tongue was very foul, but moist ; the want of rest from the agony of the pains was most distressing. The attack was of three days' duration when I first saw him. I immediately put him on the opiate treatment ; he first got one grain every two hours ; the quantity was then increased to a grain every hour ; and this was continued for thirteen days with the administration of an occasional purgative. On the fourteenth day he began to take the *mist. guaiaci c. sulph. quinae* ; and on the fifteenth day he was walking about his parlour, complaining only of not being as strong as usual, but free from pain and swelling ; and he described the treatment as most grateful to his feelings, the pains being so much lulled by the opium, that he passed through the attack with very little suffering. In reply to some inquiries of mine, as to his impressions relative to the opiate plan of treatment, he favoured me with the following :

“ Mount Michael, Glasnevin.

“ DEAR DOCTOR,

“ You ask me for my recollections of rheumatism ; they are not very agreeable, but I owe you the performance of a task, more disagreeable than that of recalling the remembrance, as some return for the comfort your treatment afforded me when labouring under the reality.

“ I suffered eight years previously under two successive at-

tacks of rheumatic fever, from each of which my recovery was tedious, extending through a period of some months.

“During the month of April this year, my strength was much exhausted by the exertions of the preceding winter. I was low-spirited, weak, and hypochondriac. The first warning of a coming attack consisted in a sentiment of occasional faintness ; a feeling of uneasiness about the region of the heart, as if its throbbings were about to cease ; then came, during the two following days, coldness, shiverings, general soreness, and the other agreeable forerunners of fever. I had recourse to my usual panacea for all ills—a warm bath—but found myself much worse after it. Acute pains began to nestle themselves, particularly in the small joints. The next morning I drew some blood from my arm ; but getting no relief, I requested that evening your attendance.

“You came, and found my ankles and wrists swollen enormously ; while I writhed under a succession of exquisite aches, now in one joint, now in another, to which the pain of tooth-ach is elysium. You afforded me, however, some comfort by telling me my heart was untouched ; and the *primæ viæ* requiring to be cleansed, you directed me to take some purgative medicine ; which having performed its office, you desired me to commence taking one grain of opium every second hour.

“I confess that I was somewhat afraid of what appeared to me very large doses of this powerful drug, especially as my head always had a tendency to be affected whenever I had fever of any kind. It was therefore with some misgiving I obeyed you, but soon had reason to congratulate myself on the effects of your advice, for during the remainder of my illness, i. e., from the second day after being forced to succumb, the pains, although they visited me occasionally, were by no means so intolerable ; I slept much, my intellect remained clear, except when occasionally I took an overdose of the opium, (for as soon as I began to experience its good effects, I became quite enamoured of it,) and, in fine, I was enabled to walk down

stairs the fourteenth day after taking to bed. During another week I rubbed such joints as were occasionally painful with a liniment made with sulphur and camphorated oil, and took internally quinine and guaiacum; but since then, now during a period of four months, I have not had the slightest return of the disease. As nearly as I can recollect, I swallowed during my illness about two hundred grains of opium.

“Such are my reminiscences of rheumatism. To the influence of large and repeated doses of opium in relieving its agonies and shortening its duration, I gladly and gratefully bear testimony; but I must confess, that whatever may be my veneration for the ‘lights of science,’ I have not the slightest ambition of again personally testing its efficacy.

“I remain, &c. &c.

“J. ALDRIDGE.”

CASE VII.—On Wednesday, 3rd May, I saw with Mr. Shaw, a gentleman, Mr. H——, ætat. 26, who had been then three days suffering under very acute rheumatism. The shoulders, wrists, and knees, were swollen and very painful; and the exacerbation of the symptoms, sometimes in one joint, sometimes in another, had left him for three nights without a moment's sleep. His pulse was 120, full; tongue moist. I put him on opium, and he took every day for six days from eight to ten grains of opium. On the seventh day he began to take, along with the opium, mist. guaiaci c. sulph. quinæ; and on the eighth day of my attendance, eleven days from the commencement of the attack, he complained only of stiffness in the affected joints. His pulse was down to 76, and his appetite was good.

CASE VIII.—Mr. C —— had been ill for a few days with symptoms of pleuritis. For these he was bled and mercurialized, and the symptoms of pleuritis disappeared; but while convalescent from this attack, with his mouth sore, his knees, shoulders, and ankles began to swell, and acute rheuma-

tism quickly developed itself. It engaged the joints, the back of the neck, and the thorax, his respiration becoming constricted, and his countenance anxious. I ordered him first, one grain of opium every fourth hour. It was increased then to one and a half grains. At the end of three days he was nearly free from pain; he then omitted the pills, and a relapse, but not severe, set in. The opium was then given in the dose of gr. ii. every three hours for twenty-four hours, when the pains again disappeared, and he had no further relapse.

It is in my power to add to the above number of cases, but it would be, I think, unnecessary to do it; I should be only multiplying repetitions of similar details. I shall now offer some general observations on the treatment by opium, and on some circumstances connected with it.

The most important rule to be remembered in employing opium for the cure of acute rheumatism is, *that full and sufficient doses* shall be exhibited. I have heard of opiate treatment having disappointed some who have tried it. On inquiry, I have learned that in those cases it has been given only to the extent of a grain every fourth or every sixth hour. This is not "the treatment of rheumatism by opium;" it is making the patient worse than before; it is inflicting on the patient the mischief arising from the stimulant effects of the drug, and withholding from him all the benefits of its sedative influence. The opium should always be increased in dose, both in frequency and quantity, until the patient feels decided relief; and should be then kept up at that dose until the disease is steadily declining. The first indication that tells the practitioner, he has reached the proper dose, is, the statement of the patient, who in reply to an inquiry as to how he has passed the night, probably says that he has not slept, but that he is free from pain and feels comfortable. This effect having been attained, the opium may then be continued in repetitions of the same dose as to frequency and quantity. Clarke's Case, No. VIII. shows the rapidly good effects of the large doses. A relapse set in: he got two grains of opium every third hour,

uninterruptedly for twenty-four hours. He took sixteen grains of opium within twenty-four hours, and the relapse was suddenly cut short. In Rooney's Case, No. III., in the first day eight grains of opium were administered. The dose was then increased to twelve grains within the twenty-four hours, for the second, third, and fourth days; and on the fifth day he was taking bark. In Dr. Aldridge's Case, the quantity taken in a fortnight amounted to about 200 grains. I think about ten or twelve grains in every twenty-four hours, will be found the average quantity required. The tolerance of the remedy is a remarkable feature in the treatment, and may, I think, be fairly adduced as an argument in favour of its propriety. The head is not affected by the large quantity of opium administered. This is remarkably shown in Dr. Aldridge's Case, (Case VI.) where the head was not injured by the opium, even though there had been previously a tendency to derangement of cerebral functions in all previous febrile affections. There is another singular circumstance connected with the exhibition of the opium. It is the occurrence of diarrhoea while the patient is using the opium even in full doses; in some instances, the diarrhoea becoming so troublesome as to require starch enemata, or chalk mixture, with kino. It is seldom necessary to purge the patient while administering the opium; indeed the pains are sometimes brought back by the administration of a purgative, either from the patient catching cold in rising from bed, or from the irritability produced by the action of the purgative. The patient's bowels, if they have not been constipated in the commencement of the attack, may be not only safely, but with benefit, not disturbed more than once in two days.

In the greater number of the cases, in conjunction with the internal use of opium, I have employed embrocation to the affected joints; these consisted either of warm spirits of turpentine, applied by means of flannel soaked in it; or when this has been too irritating to a delicate skin, camphorated spirit: in a few cases, simple decoction of poppy heads was the most grateful ap-

plication. When the constitutional disturbance had passed away, and that the local uneasiness had nearly, if not altogether subsided, slight stiffness, with little or no swelling remaining, a liniment of equal parts of ol. terebinth and ol. camphorat. with ʒi. of sulphur to each ounce of the liniment, was useful in removing the last remnants of the affection. There arises in the progress of a case of acute rheumatism a stage that is sometimes perplexing. The fever seems very much abated, the skin is covered with almost constant perspiration, even sometimes to the degree of producing a miliary eruption, and so profuse in quantity that the patient, when the clothes are raised, steams like one in a vapour bath, and the skin becomes clammy, pale, and soddened; the pains become erratic, and the pulse becomes quicker and smaller: in such a stage, and with these symptoms, the conjunction of sulphate of quinine with opium is the combination that, I think, will be found most beneficial. K——'s Case (No. V.) is an example of such as I have described, and shows the good result of the exhibition of the combination of quinine and opium in that particular stage. Another remedy of considerable efficiency, when the acute stage is passing away, is the mist. guaiaci. The preparations of guaiacum have been an old and favourite popular remedy in rheumatism, and have fallen, perhaps undeservedly, into comparative disrepute. As it is not my wish to travel beyond the acute form of the disease, I shall make no observation on the use of the hydriodate of potass, or of any of the remedies adapted to rheumatism when passing into a chronic state. There is one form of acute rheumatism in which the opiate treatment will cause disappointment, should the practitioner trust to it alone. It is that form which we sometimes meet with, in persons whose habits of living, and perhaps hereditary tendency, give a predisposition to gout, and in whom, when rheumatism does appear, it is not genuine rheumatism, but a combination of gout with rheumatism.

The relative values of the treatment by opium here recommended, and of the treatment by bleeding, and antiphlogistic remedies advocated by Bouillaud, &c. may be considered under

two heads : *first*, with reference to the purely practical merits of the respective lines of treatment, without regard either to the intimate nature of the disease, or the *modus medendi* of the remedy ; *secondly*, as to the accordance of those respective modes of treatment with general principles of pathology and therapeutics.

As to the *first* head, viz. the practical merits of the opposing lines of treatment, the question is one of pure empirical experience. The Profession will judge for themselves as to the powers of those respective modes of treatment to attain the objects which are to be kept in view in the management of acute rheumatism. In treating acute rheumatism, the objects which are anxiously sought for, are : 1st, shortened duration of the attack, and alleviation of the suffering of the patient ; 2nd, convalescence, with strength as little diminished as possible ; and 3rd, prevention of the complications of endocarditis, pericarditis, &c., which, when they occur, are so often in after years the cause of prolonged suffering, ending only in death.

1st. As regards the duration of the attack ; on examining the cases I have related, we find that in round numbers, the average duration of each case was seventeen days from the commencement of the attack to its termination ; and reckoning from the date of commencing the opiate treatment, the average duration was only nine days.

I have examined the cases related by Bouillaud, treated by depletion, and taking an equal number of cases related by him, (I have taken the first eight,) the average duration is thirty-five days from the commencement to the termination ; and reckoning from the date of his commencing the treatment by bleeding, the average duration of each case is twenty-six days. This comparison is greatly indeed in favour of the power of the opiate plan of treatment to shorten the duration of the disease. The comparison should indeed, in strictness, give a result more unfavourable to Bouillaud's plan of treatment, for the duration of some of his cases appears to have been

much longer than his account makes them. Thus in Case III. the patient is marked as dismissed on the 5th of October; but she does not appear to have been quite recovered;* and while a rheumatic pain remains, the patient cannot with safety be marked as cured. In the relation of Case I. the last note marks the patient as suffering under pleuritic effusion;† and in Case VIII. recovery was indeed very doubtful, for the patient is described as very well, but with rather an ominous exception—*bruit de soufflet* in the præcordial region.

In regard to the alleviation of the patient's sufferings in passing through an attack of acute rheumatism, I think a very little observation will convince the most prejudiced, of the superiority of the opiate treatment. The patient under its influence is not narcotized, but he describes himself as comfortable; and even when the night is passed without a sleep, it is passed in calm quietness, very different from the moaning agony of a patient treated on the depleting plan.

The *second* object sought for, namely, the convalescence of the patient, with strength as little diminished as possible, may now claim a few observations. This object is not inferior in importance to the recovery itself of the patient. There is no disease more likely to relapse than rheumatism, and we have really done little good for the labourer or artisan whom we have cured of rheumatism, if we have reduced him, in curing him, to such a state of debility, as to render him more liable than he was before, to catch the disease; if we have made him less able to resist the injurious impression of cold. On this head I have a very decided conviction in favour of opium. The strength is very little, if at all, reduced by it; on this point Dr. Aldridge's evidence, who suffered from more than one attack of the disease, is very conclusive. A patient recovering under the use of opium

* "Elle dit éprouver quelques douleurs dans les épaules," &c.

† "La respiration est précipitée, (32 respirations par minute,) la parole entrecoupée," &c.

has only to recover from the debility and stiffness which are more or less inseparable from the disease. A patient treated on Bouillaud's plan has to recover from what is worse than the disease, the debility, which is the necessary result of the frequent bleedings *coup sur coup*, of cupping, tight bandaging, blisters, and mercurial cerates, for he uses all those adjuvants as he call them ;* and even then on recovery the slightest cold will, on Bouillaud's own admission, produce a relapse.† The patient is thus, not only liable to a relapse of the disease itself, but he is, as Sydenham has observed, not only weakened for a time, but is rendered by his debility more obnoxious to other diseases for years to come: the patient cured by opium has neither bleeding, blistering, nor mercury, to recover from in his convalescence. The opiate plan of treatment attains the second object, viz. the convalescence of the patient with strength as little diminished as possible.

When we pass now to the *third* object, the prevention of the complication of endocarditis, pericarditis, &c., we have, I think, still more reason to cling to the opiate treatment. These complications of rheumatism are the terror of the practitioner ; when they occur, they almost invariably, even under the most judicious and most persevering management, entail years of suffering to end only in death. With regard to the power of preventing such complications, the opiate treatment takes a station, I think, immeasurably beyond all others. Of the eight cases I have enumerated, the heart has not been affected in a single instance. In some of them the danger of the heart becoming engaged seemed imminent, but perseverance in the plan laid down baffled the attack. On examining my note book both of hospital and private patients, I find out of all I have treated by

* " Les moyens adjuvans des emissions sanguines coup sur coup sont, la diete, les boissons emollientes, les vesicatoires, la compression exercée autour des articulations malades, l'application de compresses enduites de cerat mercurial," &c. &c.—p. 137.

† It is necessary "d'éviter avec le dernier soin, le plus léger refroidissement." —p. 137.

opium from the commencement, but a single instance where the heart became engaged. Compare the results of this with Bouillaud's account of the frequency of pericarditis, &c. in the cases treated on the bleeding and antiphlogistic method. He says in his preface, p. 2, that pericarditis or endocarditis is an almost constant accompaniment of acute rheumatism;* and in another place, p. 27, "eight times out of nine, acute rheumatism has been accompanied with an attack upon the fibrous tissues of the heart." This is a fearful commentary on the treatment recommended by him; and were there no other recommendation in favour of the opiate treatment than a comparison of the results of it, compared with the results of the treatment of Bouillaud, this alone would be sufficient to confer on it immeasurable superiority.

I have now stated the results of the treatment by opium, in relation to the three practical points of such importance in the treatment of rheumatism, viz. the shortening the attack, with alleviation of the patient's sufferings; the convalescence of the patient with little diminution of strength; and the prevention of the complications of pericarditis, &c. &c. And on comparing the results with the results of an opposite line of treatment, I think the treatment by opium stands decidedly superior. In these observations I have confined myself strictly to the mere practical question, of treatment without regard to any theory, either of the nature of the disease, or of the action of the remedy. I shall now devote a few observations to an examination of the accordance of the administration of opium, with the principles of general pathology and therapeutics.

The advocates on principle of the antiphlogistic treatment of acute rheumatism, assert either as Sydenham and Bouillaud, &c. that the disease is inflammation of the joints; or as Roche, Andral, &c. that plethora, with alteration of the nature of the

* "—— coincidence a peu pres constante, soit d'une endocardite, soit d'une pericardite, soit d'une endo-pericardite avec un violent rhumatisme articulaire aigu."

blood is the cause of the disease, and that therefore the treatment which must and ought to be adopted is bleeding, and rigid antiphlogistic management. Roche says,* that the supposition of its having its cause in an alteration in the nature of the blood, readily explains the rapidity with which rheumatism flies from joint to joint, the facility with which it seizes on the fibro-serous tissues of the heart, the intermissions and the relapses of the disease. He does not give the chain of argument by which he arrives at this explanation, nor can I make the least approach to discovering it. On the contrary, were the supposition true, of rheumatism depending on alteration of the nature of the blood, I cannot understand why it should fly from joint to joint while all the vessels of the body necessarily contained the same altered blood ; nor why it should seize on the membranes of the heart ; nor how it could have intermissions and relapses, while its cause, the same altered blood, was still continuously circulating. If the supposition has no good theory to support it, it cannot then offer any valid reasons for pursuing an antiphlogistic treatment, nor present any valid objection to a contrary line of practice.

Let us turn now to the argument of Bouillaud, &c. that acute rheumatism is nothing but ordinary inflammation, and that, therefore, the treatment must necessarily be what he advocates.

The arguments in favour of its being common inflammation, are, that in the parts affected there are pain, swelling, turgescence of the vessels, &c. It is argued thus : we say a part is inflamed when these phenomena are presented in it ; these phenomena are presented by the joints in acute rheumatism ; therefore these joints are in a state of common inflammation. It is true that we do apply the vague term *inflammation* to those phenomena ; but it is no less true, that these phenomena may be present, and yet a part may not be in a state of inflammation in the ordinary sense of the term. Andral's division of primi-

* Art. Arthrite, Dictionnaire de Medicine, &c.—pp. 421, 457.

tive vital actions will help us in our explanation. When a part is inflamed, in the ordinary sense of the term, there is a lesion of circulation in the part, which causes the redness, congestion, and turgescence ; there is a lesion of the functions of nutrition and secretion in the part, on which depend the effusion, &c. ; and lastly, there is a lesion of innervation, or the function of sensibility, from which arises the pain. In ordinary local determinations or inflammations, all these functions seem to be simultaneously deranged, or the derangement of the circulation holds a prominent place, and we are then obliged to use depleting measures ; but there is another class of local congestions or determinations in which the nervous sensibility of the part becomes first deranged, a “ lesion of innervation ” takes place ; this will not long continue without involving the other primitive vital functions of the part, and then, when all are deranged, we have, it is true, all the phenomena of inflammation present ; but the order in which they have set in, is different from ordinary inflammation, and in a practical point of view, the order of succession becomes an element of great importance. Two patients may have intolerance of light, and violent headach, flashing of light across the eyes, turgescence of the vessels of the face and head, and throbbing of the temporal and carotid arteries. All the phenomena of inflammation may be present in both, but yet their essential and intimate natures are different ; the one is treated by active and repeated depletion, the other is cured by the administration of a stimulant or opiate. In the latter, the lesion of nervous sensibility, or the derangement of nervous function, is first in order ; the vascular and secretory system are then implicated by sympathy and intimate connexion, and the combined derangement of the three functions presents the phenomena of inflammatory action ; but the primary cause being discovered to exist in the lesion of innervation, this is acted on by an opiate or stimulant, and the other lesions, which are as an effect dependent on it, then disappear.

The phenomena of acute rheumatism present analogous

characters. The disease sets in with pains in one or more joints ;* these pains present most of the characters of neuralgic pains ; they are flying from joint to joint, and even in the same joint are at different times of the most varying degrees of severity, at one time rarely felt, in another hour intolerably severe. Common inflammation of a joint, in which all the primitive vital functions of the part are equally affected, does not present such characters ; it does not leave one joint to attack another, it will not suddenly vanish and suddenly reappear, and having reached the degree of pain, swelling, and effusion, which we see in acute rheumatism, it will not subside, leaving no organic alteration of structure behind it.† The difference between rheumatism and common inflammation is well known and acted on in rheumatic inflammation of the sclerotic coat of the eye ; the same distinction is applicable to affections of the joints, whether one or more be engaged. The characters which belong to acute rheumatism of the joints, are sufficiently well marked to stamp the affection of the joints, even in a very high state of pain, swelling, and effusion, as very different indeed from common inflammation ; and to bring them within that class of diseases in which the lesion of innervation is the primary cause, and in which, treatment is therefore to be directed primarily to correct that lesion of innervation ; and hence the administration of opium in this disease is in strict accordance with correct principles of pathology and therapeutics. The theory of altered innervation will explain the sudden transition of the pain, swelling, &c. from part to part ;

■ Cullen is almost the only writer who gives an accurate description of the onset of an attack of acute rheumatism. Most other writers describe the disease as setting in with a regular premonitory stage of general fever, which is *succeeded* by the pains. This is not accurate ; Cullen correctly states, that “more commonly pains are felt in particular parts, before any symptoms of pyrexia appear ;” and that “a joint after having been for some time affected with pain, commonly becomes affected also with some redness and swelling which is painful to the touch.”

† In some fatal cases of rheumatism, effusion of purulent matter has been found in the joints. There are exceptions ; when occurring they are similar in kind to the effusion into the pericardium, which we shall notice further on.

while the theory of the disease having its seat in an altered state of the blood, or in general plethora, fails in the explanation.

The general fever which accompanies rheumatism, is not the fever of common local inflammatory action. A patient in acute rheumatism, with a pulse of 130, and the fever apparently very high, is bathed with partial and copious sweats; we never see this in the constitutional fever of common acute local inflammation of equal intensity. A moisture on the skin, in the ordinary fever of acute local inflammation, is a sure sign of the disease abating. In rheumatism it is as often a sign of aggravation: occasionally, even, as in K——'s case, No. V., the whole surface is teeming with perspiration, while the local pains are worse than ever. Indeed, the fever of acute rheumatism is the fever of irritation, not the fever of common local inflammation.*

An argument, at first sight apparently strong in favour of placing acute rheumatism in the category of common inflammation, has been drawn from the changes produced in the pericardium, when on examination after the death of a patient caused by metastasis to or engagement of the pericardium, there is found effusion of lymph and purulent matter, such as is found in common inflammatory pericarditis. A little consideration will, I think, show that this argument has no weight. If the tendency of acute rheumatism were, as its ordinary consequence, to cause effusion of lymph, purulent matter, &c. it ought to cause it as well in the joints as in the pericardium; but it does not. The effusions that are found in the pericardium do not then prove that such effusions are the general and ordinary consequence of rheumatic inflammation; they only prove that the pericardium, when attacked, is liable to such changes; and the occurrence in rheumatism of those effusions into the pericardium, admits of an explanation which is referrible to the peculiar

* It will happen in the fever of acute rheumatism, as in other irritative or neuralgic fevers, that a single abstraction of blood may be required as a preparatory step to the administration of opium; just as in irritative ophthalmia, &c. a slight depletion is used as a preparation for a tonic plan of treatment; but as it is very rarely required, and only when there is *very unusual aggravation* in some individual case of exception, it is not necessary to do more here than allude to it.

circumstances of the pericardium, and to the functions which it performs. The explanation, I believe also, has not before this, been offered. When a joint is attacked by acute rheumatism it is preserved perfectly at rest; the instinctive desire to avoid motion is so great, that the patient remains immovable in the bed. If one of those joints were, however, forcibly kept in motion for several hours, and its irritated surfaces grated against one another, it is easy to conceive that aggravated inflammation, and now of the ordinary kind, tending to terminate in suppuration, &c. &c., would be superadded to the previous acute rheumatic affection; what does not occur in the joints, namely, injurious motion, is necessarily kept up in the pericardium. The action of the heart must continue, it is even exaggerated by the constitutional fever; the two layers of the pericardium are kept in constant irritating friction against one another, aggravated inflammation of a disorganizing character is now superadded to the previous rheumatic affection, and the result is the effusion of lymph, pus, &c.* In this view of the explanation of the occurrence of pericarditis, &c. in acute rheumatism, the action of opium in preventing it, is easily explained; we cannot arrest the motion of the surfaces against one another, but we can lessen the exaggerated innervation of the part; we can lessen its irritability; we can, as it were, by blunting its sensibility, render it less liable to be irritated by that motion, and thus render it less liable to run into ordinary inflammation. We cannot diminish the action of the efficient cause, viz. the motion, but we can render the organ better able to bear it; we can do this by opium; and, therefore, I believe it is that the opiate treatment becomes the best preventive of the affections of the heart in acute rheumatism; while the treatment by bleeding, by increasing irritability, renders the pericardium less able to bear the exciting cause, viz. its own motion, and therefore more liable to be excited to run into common inflammation.

* The opiate treatment is of course no longer sufficient, when common inflammation has supervened on the rheumatic affection.

I have now examined the question as to the merits of the treatment of acute rheumatism by opium, both in relation to the merely practical consideration of the treatment, and as to its accordance with general principles of pathology and therapeutics. And while as to the latter my reasoning may be disputed, the facts I have stated under the first head will, I trust, be confirmed by observation, and will obtain for the opiate treatment a fair and sufficient trial.

ART. XIV.—*On a new Chemical Combination of Arsenic, Mercury, and Iodine; and on its Employment as a Therapeutic Agent.* By M. DONOVAN, Esq.

ABOUT thirteen years since the iodide of arsenic was introduced on the continent as a therapeutic agent; but was little known as such in the British isles until lately, when it was recommended by Dr. A. T. Thomson in the cure of psoriasis.

Judging from the medical effects of arsenic, mercury, and iodine, coincident as they are in some diseases, several practitioners have used them in conjunction. Circumstances led me to make experiments on the chemical agency of these three substances on each other; and as the results appear likely to prove useful, I am induced to publish them.

The red iodide of mercury has been shown by Bonsdorff to act in some respects as an acid, and to be capable of combining and forming crystallizable salts with several compounds which, in these cases, act the part of bases. These salts he denominates iodo-hydrargyrates.

I observed that when iodide of mercury, and iodide of arsenic, are triturated with as much water as is sufficient to render them moist, the scarlet colour of the former, and the brown colour of the latter disappear, and the resulting colour is yellow; a sufficient indication that a chemical combination has taken place.

I found also, that when iodide of mercury, and iodide of

arsenic, mixed, are heated in a glass tube, they melt, and form a compound so volatile, that, at a moderate temperature, it boils, evaporates, and produces concussions of the tube. This compound is very fusible: on cooling, after being melted, it concretes into a reddish-brown, crystalline mass. Its vapour has the property of tinging flame of a pale violet hue.

The liquid obtained by presenting the crystalline mass to a large quantity of water, is yellow. When this solution is evaporated by heat, the salt passes off with the water, such is its volatility. Towards the end of the process, however, the solution becomes more concentrated, and its colour deepens to brown. If the brown liquor be now distilled in a retort, the water and salt still come over together, and the liquor in the receiver is also brown. A small quantity of light, shining, minute crystalline plates appear in both retort and receiver.

From these facts, it is plain that the crystalline, reddish-brown mass, obtained by combining the iodides of mercury and arsenic, is an iodo-hydrargyrate of arsenic, according to the nomenclature adopted by Bonsdorff.

The medicinal effects of the three elements, iodine, mercury, and arsenic, of which this salt is composed, appear from the testimony of several writers, to coincide in their influence over certain diseases, as lepra, several forms of psoriasis and lupus. A chemical combination might possess increased efficacy: there is no doubt that it would be convenient, and that it would afford a greater security and certainty, in exhibition, than the three elements administered in any other form. The mercury is here in such a state of combination that it may be exhibited in solution: and it is probably to the solubility of corrosive-sublimate, and hence to its absorbability, and transmissibility to the extreme vessels, that its superior power of quickly arresting the progress of syphilis, and of acting on the skin, is attributable. By the same causes also, the analogous effects of bromide of mercury may be produced. Pearson considered corrosive-sublimate “peculiarly efficacious in promoting

the desquamation of eruptions." Willan believed it the only mercurial that is of any use in lepra. Perhaps the iodo-hydrargyrate of arsenic, as containing mercury in the soluble state, may support the same character, and even exceed it, by containing other useful elements. Corrosive-sublimate has been hitherto the only mercurial that could be exhibited in solution.

But there are obstacles which embarrass the exhibition of the solid combination of iodine, arsenic, and mercury, some of which arise from the difficulty of obtaining, for its preparation, pure iodide of arsenic of unvarying strength; and others, from its own nature.

The iodide of arsenic has been prepared by various processes. M. Henry merely melts 100 parts of iodine with 16 of arsenic. MM. Serullas and Hottot recommend the same method; but add, that the mass should be sublimed. M. Plisson boils 100 grammes of iodine, 30 of levigated arsenic, and one litre of water, until all smell disappears, and the liquid becomes of a light yellow colour. The solution, filtered when cold, is to be evaporated with free access of air; crystals will form, and when they are very nearly dry they are to be melted by heat: a feeble heat will expel all the water. In order to obtain a neutral compound, all the crystals which form on the side of the capsule must be continually returned into the liquid. (Plisson; *Annales de Chimie et de Physique*, xxxix. 265.)

If Henry's process be inadequate without sublimation, Plisson has shewn that sublimation causes decomposition of a portion of the iodide into metallic arsenic and iodine. And there is this difficulty in Plisson's process, that there is no obvious criterion whereby the total expulsion of the water can be judged; hence the iodide is liable to variation of strength. I have found in the iodide of arsenic prepared for sale, so much as one-twentieth of its weight of metallic arsenic. When such iodide of arsenic is dissolved in water, by means of iodide of mercury, the metallic arsenic remains undissolved.

An iodo-hydrargyrate of arsenic and mercury, formed from such an iodide of arsenic, will be liable to uncertainty of strength.

When iodo-hydrargyrate of arsenic is intended for administration in the form of pills ; or when pills are formed (as is sometimes done) by combining the iodides of arsenic and mercury ; there are other sources of uncertainty, which cannot fail to produce detrimental effects. One arises from the difficulty of weighing and mixing such minute quantities of the materials without loss ; of diffusing them equally through a pill mass, and dividing it into pills containing perhaps but one-sixteenth of a grain of each ingredient. Add to this, the chance that some injury is done by the substance employed to give bulk, or to form a mass, and the certainty that spatulas, and other metallic instruments, whether of brass, silver, or steel, will produce decomposition of these minute quantities to a considerable extent. All these considerations evince the inexpediency of this form of exhibition. It is even questionable how far a pill is safe consisting of three escharotic substances, exerting their concentrated power on a single spot of the stomach, in consequence of their being in the solid state.

The form of solution is by much the best when proper precautions are taken. In order to render the exhibition of these active ingredients safe, the formula must be capable of furnishing a solution, which, when prepared by different hands, from different materials, and at different times, will always be of the same composition and strength. Without this quality, a practitioner, who has urged the dose of a weak solution, may, by persevering in the same dose with a strong one, do great mischief.

To insure this equality of composition, and therefore of strength, at all times, the first condition is, that the three component ingredients shall be perfectly pure. The metallic arsenic of commerce should be powdered and sublimed from black flux : the iodine should be washed with water, strongly pressed between folds of bibulous paper, and sublimed : and the mer-

cury should be recovered from red precipitate by distillation.

The next requisite for constancy of composition is, that the quantities of the ingredients employed must be entirely dissolved in the solution : we must be sure that the three substances are in full operation to the fullest extent of their quantity.

Lastly, the compound must be permanent, not liable to spontaneous decomposition, nor to injuries arising from careless exposure to light or air.

I believe that all these conditions are fulfilled in the following process :—

Triturate 6.08 grains of finely levigated metallic arsenic, 15.38 grains of mercury, and 50 grains of iodine, with one drachm measure of alcohol, until the mass has become dry, and from being deep brown has become pale-red. Pour on eight ounces of distilled water ; and after trituration for a few moments, transfer the whole to a flask ; add half a drachm of hydriodic acid, prepared by the acidification of two grains of iodine, and boil for a few moments. When the solution is cold, if there be any deficiency of the original eight ounces, make it up exactly to that measure with distilled water. Finally filter.

The theory of this process need scarcely be adverted to. By the long-continued trituration of arsenic, mercury, iodine, and alcohol, the metals are converted into iodides, which combine. The mass by solution in water is converted into an hydriodate of arsenic and mercury. The quantities of the two metals are so adjusted, that, when converted into protoxides by decomposition of a portion of the water in which they are dissolved, there will be eight grains of protoxide of arsenic, and sixteen of protoxide of mercury. The quantity of water is such that each drachm measure of the solution will contain exactly one-eighth of a grain of protoxide of arsenic, and one-fourth of a grain of protoxide of mercury. I conceive that the quantity of mercury ought to be double that of the arsenic, in

order to insure a slow and moderate, yet adequate mercurial action, along with the proper effect of the arsenic.

Of this *liquor hydriodatis arsenici et hydrargyri*, each drachm measure consists of—

Water, one drachm.

Protoxide of arsenic, one-eighth of a grain.

Protoxide of mercury, one-fourth of a grain.

Iodine, (converted into hydriodic acid,) four-fifths of a grain.

The colour of the solution is yellow, with a pale tinge of green : its taste is slightly styptic. It cannot be properly conjoined with tincture of opium, or with sulphate, muriate, or acetate of morphia ; for all these produce immediate and copious precipitates in it. Hence if opiates are to be used during the exhibition of this arsenico-mercurial liquor, they must be taken at different periods of the day. Tincture of ginger produces no bad effect. The following formula is proper :—

℞ *Liquoris Hydriodatis Arsenici et Hydrargyri* drachmas duas.
Aquæ Distillatæ uncias tres cum semisse.
Syrupi Zingiberis semunciam. *Misce.*
 Divide in haustus quatuor. Sumatur unus mane nocteque.

Thus one-sixteenth of a grain of protoxide of arsenic, and one-fourth of a grain of protoxide of mercury, would be taken in each dose, along with two-fifths of a grain of iodine, which being in the state of combined hydriodic acid, will be much diminished in energy of medical effect. This is no doubt the proper dose to begin the exhibition of arsenic with ; but it will be very soon necessary to increase it.

The division into draughts is here necessary : first, to insure accuracy of the dose, so essential in the case of this active medicine : and next, to prevent injury to the ingredients by the use of a metallic spoon as a measure—the general way in which, unfortunately, the dose of a medicine is determined.

ART. XV.—*Notice concerning a Case of Cholera, and a Case of Diffuse Inflammation.* By J. KIRBY, M.D.

TO THE EDITORS OF THE DUBLIN JOURNAL OF MEDICAL SCIENCE.

64, Harcourt-street, Sept. 27, 1839.

GENTLEMEN,

I conceive it to be my duty to apprise the Profession, through the medium of your periodical, that I have had lately under my care, a case of genuine Asiatic Cholera, in the person of a lady who suffered from severe diarrhoea for several days. Finding the disease to continue, and her debility to increase, she removed from country lodgings to her town residence, where I saw her the next day in consultation with Dr. Price.

The dejections were profuse, and of specific character; the stomach rejected such fluids as were taken to allay inveterate thirst; the extremities, forehead, nose, and tongue were cold; the face was collapsed, and of a bluish tinge; the hands were of the same colour, and they presented a shrivelled appearance. Distressing cramps attacked the abdomen and inferior limbs. Pulse frequent, small, and easily compressible; respiration hurried; voice sunken; much general anxiety; a clammy sweat bedewed the surface of the body; the urine was suppressed, and continued so for three days.

It is more than pleasing to be able to state, that the subject of this case recovered, convalescence having being established on the eighth day.

From my personal experience of the curative virtue of acetate of lead during the last invasion of this formidable disease, I had recourse to that remedy. I administered it in doses of two grains, with two minims of Battley's sedative liquor; at first, every half hour, and subsequently every hour. At the same time employing injections every third hour, composed of starch mix-

ture, a few drops of laudanum, and a scruple of the acetated cerussa.

Under this treatment the stomach became settled, and the diarrhoea ceased in a short period. She was now permitted to take small quantities of cold sherry and water, an indulgence equally indicated by existing debility, and the eager desire expressed for that particular beverage. Means were judiciously contrived by Dr. Price to preserve the temperature of the patient, and strong camphorated liniment was frequently rubbed on all parts invaded by cramps during their continuance.

From this case I leave your readers to deduce their own inference ; I have only to say, that it strengthens my confidence in a remedy, the sufficient efficaciousness of which was already much established in my mind.

Gentlemen, your obedient servant,

J. KIRBY.

TO THE EDITORS OF THE DUBLIN JOURNAL OF MEDICAL SCIENCE.

GENTLEMEN,

If the accompanying case appear as singular to you as it does to me, I am sure you will not hesitate to give it such a place as it seems to deserve in the pages of your periodical. Was I to introduce it to notice under a particular heading, perhaps that heading should be thus worded—*a case of death from a poisoned wound, in which there are many unusual circumstances and appearances.*

CASE.—On the 13th October last, I met Dr. Churchill in consultation on the late Mrs. Wall, of Mill-street. A few days previously she was favourably delivered of a healthy child. She complained of uneasiness and swelling in the right axilla, two days after her confinement. But these symptoms were so much subdued by application of leeches, that Dr. Churchill signified he would not visit the next day, unless some unexpected circumstance should require his presence. Next morning his atten-

dance was demanded ; finding some unusual appearances, he suggested the propriety of further professional advice, whereupon my assistance was required.

When we entered the bedchamber Mrs. Wall was lying on her back, supported by pillows, and rather inclining to the left side, with the legs drawn upwards into a somewhat sitting posture. Her lips extremely livid ; the malar regions suffused, and of an intense mulberry colour ; the face cold and moist ; while the extremities, especially the upper, were bathed with a profuse and chilling sweat. The hands were shrivelled, barely warm, and of a hue as deeply purple as I have ever seen them in the most malignant form of Asiatic cholera. Tongue rather clean, moist, and no immoderate thirst. Respiration nearly natural ; but the pulse was very weak, and beat upwards of 100 in a minute.

The right arm, axilla, and breast permitted a free and almost painless examination, although an extensive patch of brown redness reached from the axillary fold of the latter to the posterior part of the trunk. The seat of discoloration was scarcely elevated, and was barely tender to a heavy touch. There was the slightest œdema, which appeared to be wholly confined to the true skin. A similar but a more extensive discoloration existed below the inferior angle of the right scapula, and another of twice the circumference occupied the lumbar region, and was more perceptibly sensitive. Lower on the loins, and over both the nates, detached maculæ of a brownish colour were profusely scattered. These were for the most part circular ; but many were of an irregular shape, while all were obviously elevated above the adjoining integuments, which were apparently healthy, both between the larger patches, and these desegregate maculæ.

Attracted by these unusual appearances, and while engaged in an inspection of them, I asked Mrs. Wall whether she had received any local wound, and if she had a sore on any part of the hand or arm. Two nurses, her attendants, replied that there was nothing such, and our patient for a moment cor-

roborated their testimony. However, in a few minutes, she recollected that she had a "trifling scratch" on her thumb, received a few days previous to her confinement. As the injury produced only a transitory uneasiness, she disregarded it, yet she remembered to have mentioned something about it to her family, who naturally considered it to be a matter of no consequence.

On the second joint of the thumb we found a circular vesicle, nearly the size of a four-penny piece; it was free from pain, and without surrounding inflammation. The centre of the decaying vesicle was brown, and depressed below the level of the circumference, which was moderately distended with an opake, milk-coloured fluid. This appearance led to an examination of the arm, which was free from pain, vesication, and from every engagement of the veins, absorbents, and cellular tissue.

The mind was undisturbed; the lochial discharge had ceased; but lactiferous action had not displayed itself in the breasts.

On the 14th, (second day,) our patient was sitting up in bed. Countenance as the day before; and the intellect perfect; the eye clear, although she passed a very uneasy night; and the expression of her countenance was that of calmness, with which her manner most thoroughly corresponded. She complained only of weakness, which was much augmented by our inspection of the trunk, over which the diseased appearance had now widely extended; at the same time that the colour of the patch had assumed a deeper hue, and all were more evidently elevated. Tongue yellowish; thirst moderate; stomach inclined to nausea; pulse very frequent, and extremely weak; urine scanty.

At 12 o'clock, on the 15th, (third day,) coma, yet she could be roused; understood the words in which she was addressed; but she had not the power to reply, although she distinctly signified that she was not in pain. Pulse scarcely perceptible,

respiration 26 in a quarter of a minute ; deglutition difficult ; tongue clean and cold ; face blue all over, and the features sunken ; fore-arms cholera like ; vomiting. She still preserved the sitting posture, with the arms laid across the chest, and the hands gently clasped in an attitude of pious resignation. She died in the evening.

The treatment, during the short period of our attendance, was stimulant : from the first we despaired ; and although we ordered ammonia, turpentine, opium, brandy, &c., yet we felt the distressing conviction that we prescribed with painful hopelessness. It did not seem to our judgment, that there was any thing in the local appearance to indicate any particular topical operative management, or remedial application.

This case bears some resemblance to the cases of Messrs. Dease and Shekleton, as detailed by Mr. Colles in the Dublin Hospital Reports. The points of similarity are seen in the character of the pustule ; in the exemption of the absorbents from engagement ; and in the freedom of all the structures between the affected thumb and the axilla ; in the kind of feverishness ; in the mental despondency ; and in the progressive sinking, uncontrollable by the most powerful stimulants.

But in the above unfortunate case, as well as in the instances of Messrs. Hutchinson and Egan, given by the same authority, the infection seems quickly to have produced great constitutional disturbance ; while in Mrs. Wall's case, it appears that it did not display its injurious powers until several days after its application. She had no marked febrile invasion, the stomach was not disturbed until a short period before her death, nor did she at any time complain of "agonizing distress," either when the affected parts were handled or put into motion. As far as the appearances on the trunk are taken into account, there is not a point of correspondence between this case and those recorded by Mr. Colles ; for here the affected parts are the cutis, which is only slightly tumid and œdematous, slightly tender, yet firm under the pressure of the finger. In the cases of Messrs. Dease

and Shekleton, the swelling involved the cellular membranes; they pitted, were doughy, and for the greater part exhibited paleness, rather than any floridness of colour, and were studded with small and hard elevations. The intense livid discoloration of the face and forearms displays a striking peculiarity in the case of Mrs. Wall, as no similar appearances are noticed by Mr. Colles.

BIBLIOGRAPHIC NOTICES.

The Institutes and Practice of Surgery, being the Outlines of a Course of Lectures. By W. GIBSON, M. D., Professor of Surgery in the University of Philadelphia, &c. 2 vols. 8vo.

IF we may fairly conclude that the rapid and free communication between these countries and America, will produce a considerable and beneficial influence on the literature and taste of our transatlantic brethren, we must not be so self-conceited as to suppose, that we shall not be benefited in turn by the dissemination of their literary labours among us. We are now regularly supplied with the best American Medical Journals; the library of the College of Surgeons boasts the possession of many of the most approved works on medicine, surgery, and other branches of the science, that have lately issued from the American press; eminent American surgeons and physicians are becoming familiar to us. Among the first of these stands the author of the work before us, whom we shall now beg to introduce to our readers. Dr. Gibson, after having been ten years Surgeon to the Baltimore Hospital, and practised with reputation in that city, was sent for by Dr. Physick to be his successor in the chair of surgery in the Pennsylvanian University at Philadelphia. This chair Professor Gibson has held for twenty years, during most of which time he has lectured to classes averaging from three hundred to five hundred pupils. The University is one of the oldest and the largest in the United States; the total number of students in 1837 was 640, of which number 401 were medical. For the same period of time, he has been Surgeon to the Philadelphia Hospital, Blockley, which contains three hundred beds, and has given clinical lectures to such large classes, that contrary to the practice here of exhibiting the features of the case to the students at the bed-side, each interesting case is brought into the theatre, and the clinical examination and remarks made on it

there. That our author must have early possessed a high reputation, is obvious, from his having been called to succeed such a man as Physick ; that he has enjoyed extensive opportunities of surgical experience we have just shown ; a book, therefore, emanating from such a source must be opened with respect, besides it bears the stamp of the approval of his countrymen in being marked as the fifth edition. He informs us in the preface, that his first inducement to publication was the want of a text book by the students attending his lectures. The character of a text book prevails in many of the articles, which are too brief to be satisfactory ; but the details, he tells us, are to be filled up in his lectures, and to be illustrated by casts, preparations, and drawings of so gigantic a size, that every one in the class may readily see them. There are, however, several essays of great merit and originality, and many of the plans of treatment detailed in these are quite new to us, and well worth attention. We may specify the method of treating hæmorrhoids, of excising enlarged tonsils, and the treatment of morbus coxæ ; the paper on osteo-sarcoma well deserves perusal ; and his determination to abstain from operation in those cases, where the character of the disease is malignant, is indeed worthy of imitation. We are sick of reading the details of the useless operations for the removal of tumours of this class, the long gashes through the soft parts, the mallet, and chisel, and chain-saw through the hard parts, the fiery cautery applied over the huge hollow to stop the bleeding, hours of endurance of varied torture by the wretched patient ; and for what ? the inevitable return, in a worse form, of his terrible disease !

Perhaps one of the most useful chapters in the book, is that on dislocation of the humerus. Two cases are detailed in it of such rarity, but at the same time such practical utility, that we shall lay the substance of them before our readers. They are two instances of the attempt at reduction in old dislocations being followed by rupture of the axillary artery.

The first was a man, aged fifty, of intemperate habits, whose left shoulder was dislocated by the fall of a heavy chest. The first practitioner mistook the accident for fracture, and mistreated it accordingly, and the earliest attempt at reduction was near three weeks after. The most forcible, but otherwise judicious, means were tried without effect ; and the man still anxious to have a useful limb, no matter at what expense of suffering, came to Philadelphia to consult Dr. Humphrey, by whom he was referred to Dr. Gibson, two months after the receipt of the injury.

By the abstraction of a large quantity of blood, the perse-

vering use of the pulleys, &c. for about an hour and a half, till the patient was quite exhausted, the reduction was effected.

About half an hour after

“ There was a general swelling about the deltoid and pectoral muscles, which was noticed both by Dr. Humphrey and myself ; but supposing it to be an approach to inflammation, a consequence to be expected after the efforts made to restore the head of the bone, nothing was apprehended from it. The swelling, however, increased very slowly for several hours ; and although remarked by the house pupils and attendants, did not excite any alarm, inasmuch as the patient complained of little pain, and conversed cheerfully with some of his friends during the greater part of the afternoon. About six o'clock in the evening, Dr. Brinton, one of the house pupils, visited him, and hearing that he had a short time before turned over in bed in order to sleep, and struck with the unusually palid appearance of his face, was induced to suspect that some unfavourable change had taken place. These suspicions were confirmed, for, upon examination, the pulse was found scarcely perceptible, and the whole system so much sunk as to render recovery impossible. Leaving Dr. Hopkinson in charge of the patient, Dr. Brinton immediately repaired to my house, and informed me of his condition. Before I could reach him, however, he expired. The appearance of the shoulder and adjacent parts soon explained, it seemed to me, the nature of the case ; for the pectoral muscle was considerably elevated, and the skin for some distance about the chest and shoulder discoloured and ecchymosed, showing, in all probability, that some large artery or vein had been torn across, during the efforts to reduce the luxation. To determine this point with accuracy, I obtained the consent of the patient's friends to examine the body ; and at ten o'clock next morning, the dissection was made by Drs. Horner and Lawrence, in presence of Drs. Humphrey, Jackson, the house pupils, and myself.

“ Under the axillary vein, as it passes near the glenoid cavity, a large mass of coagulated blood was observed ; and on clearing this away, *the axillary artery was seen protruding with its mouth open, having been torn directly across*, and separated from its connexions. Upon further examination it was discovered that the head of the bone, at the time of the luxation, had been carried downwards into the axilla, about an inch and a half below the glenoid cavity, where it formed a white ligamentous, cup-like, socket in the subscapularis muscle, and pressing upon the axillary artery, produced such a degree of inflammation as gave rise to a copious effusion of coagulable lymph, which united the artery completely for some distance to the capsule of the joint, where it surrounded the neck of the bone. The lower part of the capsule was torn and separated from the neck of the humerus ; the upper part remained entire, and was very much thickened. The head of the bone filled completely the old sockets or glenoid cavity. Beneath the deltoid muscle there was a large hollow filled with blood, and the

whole arm, as far as the elbow, had been extensively injected with the same fluid. The os humeri was carefully dissected from the condyles to its head, and the periosteum entirely scraped off, without showing the slightest vestige of a fracture. The long tendon of the biceps was found considerably elongated, but not ruptured."

The second case was that of John Layton, ætat. thirty-five, a muscular, athletic man, about six feet high, accustomed to the use of a pint of spirits daily. The left os humeri had been dislocated nine weeks before, by a violent fall on the shoulder. Before coming under Dr. Gibson's care, four different attempts had been made at reduction, much force used, and once the body suspended upon the axilla over the top of a door. For nine days he was kept on low diet, lost ten ounces of blood daily, gentle motion was given to the arm in various directions every day, and careful friction with oils applied to the shoulder. After this the reduction was attempted.

The plan was pursued unremittingly until the 15th of March, on which day it was determined to attempt to restore the bone to its natural situation. After giving a description of the attempts at reduction, he says :

"Our efforts, notwithstanding, were continued in the most gentle and cautious way possible, sometimes by the pulleys, sometimes by the heel in the axilla, when we had the satisfaction, at last, to find the head of the bone gradually approach, and finally enter, with an audible snap, the glenoid cavity ; but not until the lapse of an hour and three-quarters from the commencement of the operation.

"The patient, after being put to bed, complained chiefly of weakness, of numbness throughout the arm, of slight excoriation of the arm-pit and bend of the arm. In other respects, he said, he was comfortable, and slept during the night without an opiate.

"March 16. I saw him at eight in the morning, and observed a general swelling over the deltoid and part of the pectoralis major muscles, but not more than could have been expected under similar circumstances. Upon pressing, however, upon the most prominent part of the swelling, I was surprised to find a distinct pulsation of an aneurismal character. During the day both the swelling and pulsation slowly increased, and satisfied me that an aneurism existed in the axillary artery. Dr. Barton saw the patient in the afternoon and drew the same conclusion."

It was determined in consultation to tie the subclavian artery, which was done the second day after the operation. On the seventh day after the patient died, gangrene of the arm having supervened. The artery was found closely adherent to the head of the bone and capsular ligament by dense cellular

or ligamentous substance, so that it appeared impossible to effect the reduction of the head of the bone and not cause the rupture of the vessel. Such had taken place; the inner and middle coats of the artery where it adhered to the bone had been ruptured, and the external expanded into a true aneurismal sac; this had burst posteriorly, and formed a diffuse aneurism. The aneurismal walls were so well defined and compact, that Dr. Gibson is inclined to think the aneurism existed prior to the last attempt at reduction, and had been then burst open.

Our author says,

“From these, and from many other cases of a similar character which I have treated within the last twenty-five years, I think I have good reason to conclude, that where no adhesion exists between the artery and surrounding parts, the operation may be done with safety; that, on the contrary, when adhesion does exist (and of this we have no means to judge,) rupture of the vessel must be an inevitable consequence, whether the reduction be effected by force or by the most gentle means.

“The conclusions which I am *now* prepared to draw (from the result of the cases of Scofield and Langton, from those reported by Flambert and from other communications to me within the last two years,) are directly the reverse of what I have stated in some of the following pages; I am now disposed to condemn, in the most unqualified terms, all attempts at restoration of *ancient* luxations of the humerus and other bones, except in cases where the patient is remarkably thin and debilitated, and where there has been little or no inflammation at the time or subsequent to the displacement.

“Two cases of *old* luxations of the shoulder, one in a mulatto woman 35 years of age, the other in a man of 40, presented themselves at the Blockley Hospital during the winter of 1835. The head of the humerus appeared in each case to have formed very close adhesions with the surrounding parts, and as the luxations had been of several months' duration, and had been followed originally by a good deal of inflammation, I declined attempting reduction, and should observe the same rule in all similar cases.”

The Cæsarian section, perhaps the most daring operation the human mind ever conceived, has been twice successfully performed by Professor Gibson. We shall conclude our notice of his work by a brief abstract of the case. The lady had been married four years, was thirty years old, and was pregnant with her third child. The two former children had been removed by cephalotomy, the antero-posterior diameter of the pelvis being so narrow (two inches) that delivery otherwise was impracticable. In this third confinement she came under the care of a very eminent accoucheur in Philadelphia, Dr. Naucrede, who thought that Cæsarian section was the only appropriate operation in her

case, and requested Dr. Gibson to perform the operation should he be of a similar opinion.

“ On Wednesday, 25th March, 1835, I received notice that Mrs. R—— was in labour; and at 3 o'clock p.m., saw her for the first time, with Dr. Naucrede and F. S. Beattie. Labour had commenced the night before, though the pains during that night, and the whole of Wednesday, were slight. The os uteri, however, had dilated sufficiently to admit two or three fingers, but the membranes remained entire. In this state of affairs it was explained to Mrs. R—— by Dr. Naucrede, and the Rev. Mr. Hughes, her pastor, that it was deemed improper, in every point of view, to destroy her child, and that it was her duty to risk her own life in the hope of saving that of her offspring. After consulting for some time with her husband, and other friends, she consented to have the operation of Cæsarian section performed. A firm table was selected, and covered with a mattrass and sheets, the patient placed upon it, on her back, and her pelvis and shoulders supported by pillows. In presence of Dr. Naucrede, Professor Dewees, Dr. Dove, of Richmond, Professor Horner, Dr. Beattie, Dr. W. Coxe, Dr. Theodore Dewees, and my son, Dr. Charles Bell Gibson, I made an incision, at the centre of the linea alba, commencing about an inch below the umbilicus, through the integuments, and extending nearly to the pubes. To save the patient pain, and to prevent this first or perpendicular cut from penetrating too deeply, I requested Dr. Horner to fold up the skin with his fingers, and while thus held I passed the knife through it with its back towards the abdomen. The superficial fascia being exposed was divided, then the tendons of the abdominal muscles, next the peritoneum, and lastly the body of the uterus, all to the extent of six inches. The uterus, however, at this stage of the operation was not cut entirely through, but a line or two in thickness of the interior of its walls, left with the view of drawing off the waters before I opened the womb, penetrated the membranes, and exposed the child. At my request Dr. Naucrede introduced a finger into the os uteri and endeavoured to rupture the membranes but could not succeed. A similar attempt was made by Dr. Beattie, which also failed. Having resumed the knife, the remaining fibres of the uterus were divided, the membranes exposed, and cautiously opened by running Cooper's bistoury for strangulated hernia, upwards and downwards, to the extent of six inches, while Dr. Horner held closely together the sides of the wound, to prevent protrusion of the intestines and the escape of any portion of the waters into the bag of the peritoneum.

“ There was a right lateral obliquity of the uterus, and the position of the child found to correspond with the third breech presentation of Baudelocque. Dr. Beattie then introduced his hand and drew out the feet, while Dr. Naucrede supported the hips and back, and removed the body, and lastly, the head of the child, from the womb. It proved a girl of large size, and apparently healthy. For some seconds however, it did not breathe, and indeed not until friction on the

chest, blowing into the mouth, and the introduction of a few drops of brandy were resorted to. The cord being cut, the child (Maria Cæsarina) was removed, and in a short time cried lustily. While Dr. Horner still kept the sides of the wound together, Dr. Beattie extracted, without difficulty, the placenta and membranes, and at the same time pushed a finger from the interior of the uterus through the os tincae, to make a free communication with the vagina. During these manipulations, two portions of intestine, each the size of a pigeon's egg, protruded on the right side of the uterus and near the upper corner of the wound. They were readily kept back, however, and did not again protrude: nor did any fluid, so far as could be observed, find its way into the peritoneal bag. No hæmorrhage took place from the removal of the placenta, nor was it necessary to secure a single vessel with the ligature. There was a visible contraction of the womb, after the removal of its contents, and the incision in it had sensibly shortened in the course of a few seconds. My attention was next drawn towards the closure of the wound, which was accomplished by three ligatures of stout silk, an inch and a half from each other through the integuments alone, a small portion of the lower part* of the wound being left open for the escape of any fluids. After being gently laid in bed she slept sound for several hours, and did not change her position in the slightest degree.

“ On the twenty-fifth day after the operation, she was enabled to sit up, the wound, with the exception of a single spot, the size of a pea, being entirely cicatrized, and finally recovered, and now enjoys, together with her child, perfect health.

“ A second time Professor Gibson was called upon to operate on the same individual, and the result was equally favourable both to the mother and child, the latter—Cæsar Augustus—at the end of three months being in perfect health.”

The Cyclopædia of Anatomy and Physiology.

WE rejoice to find this admirable work proceeding steadily, and upholding the character which its former numbers had so effectually established. Our anticipations respecting the success of the Cyclopædia of Anatomy were from the commencement most sanguine, knowing as we did the zeal, energy, and talent of its distinguished editor; and it is with no small feelings of gratification and pride that we find these qualities in our former associate and countryman have been recognized and rewarded, by comparative strangers, in the great field of the British metropolis. The 17th Number of this Cyclopædia contains several excellent articles, to which our space will not allow us to refer more particularly; but we cannot overlook the great merit of what appears to us to be the most important essay of the whole, namely, that by Mr. Adams on the abnor-

mal conditions of the hip joint. This article, evidently the result of great labour, research, and thought, bears more the stamp of originality than is usually met with in contributions to works of this kind; and following as it does the able essays by the same author on the Abnormal Conditions of the Ankle and Elbow Joints, cannot fail to elevate his character as a highly scientific surgeon. In the essay before us Mr. Adams follows the arrangement adopted in his former observations on the abnormal conditions of particular joints, and considers these states under three heads; 1. congenital malformations; 2. the effects of disease; 3. the results of accident. He commences the first head with the peculiar affection, termed by the continental surgeons congenital or original luxation of the hip joint.

"This," he says, "has not in our islands attracted the notice that it seems to us to merit. When we reflect upon the very valuable additions which have been made to our knowledge of the pathology of the articulations by British writers, and observe their silence upon this abnormal state of the hip joint, we might be led to infer that this malformation had no existence in these islands; this, however, unfortunately is not true."

He then proceeds to describe the features of this malformation, contrasting his own experience with that of Dupuytren, who had given the fullest account of this affection previous to the appearance of the present article.

"He," Dupuytren, "seems to have met with the affection more frequently in the female than in the male, in the vast proportion of twenty-two females to four males; and from his description it would appear, that he has usually found in the same individual both hip joints affected. In the cases we have witnessed we have not observed this very great preponderance of female over male cases; and although we have noticed the defect to be double in the same individual, we have more frequently observed but one joint engaged."

The author then gives an accurate description of the symptoms of this affection; and having pointed out the resemblance between it and the ordinary luxation upwards and backwards on the dorsum of the ilium, and the ordinary morbus coxæ, proceeds to explain the manner of distinguishing between them.

In describing the anatomical characters, after alluding to the absence of the cotyloid cavity, and the lengthened, flattened condition of the ligamentum teres, Mr. Adams states,

"He has usually noticed that the head of the femur has lost its spheroidal shape, and presents somewhat of a conical appearance, as Dupuytren well describes; but two other circumstances he has observed in almost all the cases he has examined. 1st. That the neck of the femur, instead of having its axis directed, as it naturally is, from behind forwards, upwards, and inwards, has in this malforma-

tion lost its usual relation with the shaft of the thigh bone, and the axis is directed upwards and almost directly forwards. 2ndly. In all the cases he has as yet seen, the head of the thigh bone, instead of being directed *backwards*, as in the ordinary luxation on the *dorsum ilii*, on the contrary, has been directed *forwards*, and has been placed beside the anterior inferior spinous process of the ilium, while the trochanter major has been directed backwards on the *dorsum ilii*."

In speaking of the probable causes of this morbid condition, the author combats the idea that a simple paralytic condition of the muscles of the lower extremity, as a consequence of the irritation from teething arising during infancy, is the starting point of disease in these cases.

In the second section the diseases of the hip joint are considered, and Mr. Adams commences it by stating :

"Much as we would wish to adopt an arrangement that the pathology of Pinel Bichat would suggest, and which comes commended to us by the experience of Brodie, we do not think that this arrangement can be strictly adhered to."

And after stating his reasons for this opinion, he says :

"We feel convinced, therefore, that in the present state of our knowledge the effects of disease on the articulation of the hip may be best considered under the following heads:—1. acute arthritis coxæ. 2. chronic strumous arthritis coxæ; 3. chronic rheumatic arthritis coxæ."

Excellent descriptions of the first two of the heads are given, which we regret we cannot dwell on ; but we cannot pass over the third without calling the attention of our readers to it more particularly.

"The writer of this article, long ago, in his lectures gave the name of *morbus coxæ senilis* to the disease in question ; but as he has since met with many instances of it occurring so early as at the age of thirty or forty, he is now disposed to substitute for this name that of chronic rheumatic arthritis of the hip joint ; and he considers it as the same disease precisely as he has elsewhere in this work described as affecting other articulations."

This is a subject which we know has engaged the attention of Mr. Adams for many years. We were present at the meeting of the medical section of the British Association held at Bristol in the year 1836, when Mr. Adams read his valuable paper on this form of disease affecting various joints, and we can bear testimony to the interest it excited, as well on account of the sound pathological views it contained, as for the number

of diseased joints which he brought with him to illustrate his positions. Being aware then that this was a subject on which our author had been laboriously occupied, we looked with anticipations of pleasure and profit to the present article, for a fuller development of his views of this disease in the hip joint, and we can safely say we have not been disappointed.

The history, symptoms, and anatomical characters of this affection are given so concisely that we are apprehensive of doing Mr. Adams injustice were we to attempt to give any extracts from this part of his paper, and we therefore refer our readers to the work itself. But the following case is so illustrative of the disease, that we give it at length:—

“ Patrick Machen, now aged seventy-seven years, was brought up as a postilion and groom, but for the last seventeen years has been quite unfit for service, in consequence of his having been afflicted with a very severe pain in his right hip, from the first attack of which he became lame, and ever since, the lameness has been slowly but gradually increasing. In every other respect his health is excellent, except that he has some wandering rheumatic pains in other joints, particularly in the right shoulder. He walks with great labour and pain, and now requires the use of a stick in each hand. In the morning his movements are stiff and confined, but they become free on exercise: in the evening of a day he has walked much the pain and stiffness are worse, and increased in proportion to the excess of exercise and labour he had undergone in the day. While he remains in bed he rests on the affected hip, and suffers no pain whatever except he suddenly turns himself incautiously. As soon as he gets up and throws his entire weight on the diseased hip joint, the pain commences. If asked in what particular part of the joint he feels most suffering, he points to the back part of the great trochanter, and to a point which corresponds with the lesser trochanter. He says, the pain shoots from these points down the front of the thigh to the knee. These pains are sometimes more severe, and sometimes less, without his being able to assign any cause for these alterations, and he cannot observe that the state of the weather has any influence on them whatever. As he stands at rest, he throws the weight of his body on the left or unaffected limb, while the right leg hangs in front, and slightly across the left, and seems to be at least three inches shorter; he leans slightly backwards, and supports himself on two sticks; as he walks, the right foot is considerably everted, and when he moves without sticks (which he accomplishes with the greatest difficulty) he places the whole sole of the foot flat upon the ground. He never, however, ventures of his own accord to move without the help of two sticks, by the assistance of which he is enabled to walk quicker; while moving along thus, the heel of the affected limb does not quite reach the ground, and the lumbar vertebræ undergo great motion. He cannot, under any circumstances, flex the thigh on the abdomen, so that when he assumes the sitting posture,

he is obliged to place himself forwards on the very edge of the seat, the right thigh remaining in the same line as the axis of the trunk, the leg usually flexed, and placed under the chair or across behind the other, and he finds the utmost difficulty in putting on his stockings and shoes. He has scarcely any motion in the hip joint. When we view the hip in front, and examine it, we see and can feel a considerable bony fulness, corresponding to the horizontal branch of the pubis; the trochanter major seems placed very high up, and is extraordinarily large, as if surrounded with ossific deposits. The thigh is somewhat atrophied, being an inch and a half less in circumference than the other, but the calf of the leg is not reduced, and the muscles seem firm; the apparent shortening of the limb, when he rests on the sound one, arises from the lumbar vertebræ being much curved to the opposite side, and the pelvis being elevated on the affected side, while the real shortening, ascertained by accurate measurement, amounts only to half an inch. If we place the patient horizontally, and attempt to communicate to the hip-joint any movement, as of rotation, flexion, abduction, a well marked crepitus is elicited, and the range of motion is found to be very limited indeed; a little abduction is admitted: rotation and flexion seem just to a sufficient degree to show that no ankylosis exists. The movements give some pain to the patient, but we can press the trochanter firmly, so as to direct the head of the bone deep against the fundus of the acetabulum, and we can even strike the heel and sole of the foot with violence, without giving the patient the slightest sensation of pain."

The third section treats of accidents. These are considered under the heads, fractures, and dislocations: the whole of this part of the subject is handled with great ability. In speaking of intra-capsular fracture of the neck of the femur, the author makes the following observations, the truth of which can be readily tested by making a longitudinal section of the neck of an adult healthy femur, and comparing it with a similar portion of an aged bone.

"Besides the loss of obliquity of the neck of the thigh bone, we find two other circumstances relative to the neck of the bone itself, rendering it very liable to fracture in the aged. I mean the expansion of the cells, by which the strength of the interior of the bone is diminished; and secondly, by the partial removal by absorption of that long bony arch of compact tissue, upon which, in the adult, depends, we believe, the principal strength of the neck of the bone," [he alludes to the under portion of the neck;] "and even in many aged subjects we find the partitions of the bony cells removed, and a large cavity, filled with fatty medulla, occupies the centre of the cervix femoris. All these alterations obviously weaken this portion of the thigh bone, and we feel very little doubt, but that when the condition of the bone above alluded to exists, even without a fall, a fracture may occur. We have noticed specimens of senile degeneration of the

neck of the femur in museums, in which the neck of the femur had been removed gradually by absorption, so that the head of the bone had approximated to the trochanters. Such specimens, when the history of the case was unknown, have, we doubt not, been from time to time adduced as evidences in favour of the possibility of bony consolidation of the intra-capsular fracture. These observations on the senile degeneration of the neck of the thigh bone, sufficiently account for the so remarkable frequency of the intra-capsular fracture in the aged subject, from the most trivial causes. The fracture, under such circumstances, should, in our minds, be looked upon more as a stage of morbid alteration, from which no amendment is to be expected, than as an accidental lesion, which the efforts of nature and the aid of surgery can be deemed adequate to repair."

The remainder of this section contains perhaps some of the most valuable part of this article; but we find that we have already exceeded our limits, and we must, therefore, take leave of our author, strongly urging an attentive perusal of the original, upon every surgeon who would wish to possess the best information upon the abnormal conditions of the hip joint.

We cannot conclude, without expressing our admiration of the wood-cuts, twenty-two in number, which illustrate this article, all of them taken from drawings, furnished by Mr. Adams, from specimens in his possession.

The Pharmacopœia of the Royal College of Physicians, Edinburgh, 1839.

THAT pharmacy is not at present cultivated throughout Great Britain and Ireland, in an equal degree to what it is in the countries of continental Europe, is a complaint frequently being made; but, as it appears to us, without sufficient foundation. The magnificent establishments devoted to this art in the principal cities and towns scattered through the united kingdoms, prove at least that it is not so utterly neglected as some are willing to insinuate. In no part of the world can chemicals be obtained possessed of greater purity or beauty—witness the nitrate of silver, and iodide of potassium; and if galenicals, such as scammony, opium, &c., are commonly adulterated, this is due (as proved by the report of a commission appointed for the investigation of this subject by the Edinburgh College of Physicians) to the operation of causes, which affect equally the pharmacians of Germany and France, as well as our drug merchants at home. Until the names of Duncan, Thompson, Paris, Philips, Donovan, Scanlan, Battley, Barry, &c. are forgotten, we ought

not to stigmatize our native country with negligence in regard to an art, one of the most important to man, whether he be savage or civilized. It was said by Fourcroy, that the preparation of medicines is one of the best sources of chemical philosophy, and one of the chief springs of its discoveries; and it cannot be denied but that we have few cultivators of pharmacy in Great Britain, capable of being ranked with Nicholas Lemery, Bertrand Pelletier, and Vauquelin of France, with the Scheeles, the Tromsdorffs, the Buchners, the Bucholzs of the North. Yet while we venerate the memories of Davy and Wollaston, and glory in the names of Dalton, Faraday, Thomson, and Graham, we need not regret that the shrine of chemistry is served by another priesthood. Nor have the cultivators of pharmacy been altogether barren; let it never be forgotten, that he who was once the apprentice of an apothecary in Penzance, was enabled, by the eagle force of genius, to flutter the vanity of the greatest of imperial despots, who willingly would have exchanged the brightest jewel in his diadem for the glory of having potassium discovered within his reign and empire. But it cannot be expected that these examples should be common; the inhabitant of these kingdoms has little to induce him, except the love of science, to sacrifice ease, health, and time, in its pursuit; the rewards which in other lands are bestowed on the philosopher and the laborious student, are here conferred on the wealthy speculator in manufacture or trade, or on the successful demagogue. We do not complain of this result, traceable as it is to habits of hereditary freedom; and as regards the department of medicine under consideration, we are inclined to felicitate ourselves on its effects, inasmuch as it prevents the too exclusive cultivation of pharmacy, and curbs the medical student from wandering too far into sciences, which are at least but collateral to medicine.

We willingly acknowledge the truth of the adage, "*artem experientia facit*;" and we feel sufficiently assured that the sciences connected with medicine are so numerous and extensive, as to be incapable of being even comprehended in any single mind; that their limits can only be successfully enlarged by a separate cultivation and divided exertion. But we cannot totally separate any one department from the rest, without producing results the most mischievous. There is no doubt but that the too exclusive study of the sciences upon which pharmacy is founded, is capable of leading to grave errors. It is liable to be forgotten that the preparation of medicines, substances proper for the cure of disease, is the great end which should be kept in view: it were well if the pharmaciens of France would always bear in mind the advice given them by M. Cap, President of the

Society of Pharmacy, in his address, published in the January number of their Journal: "Ne vous laissez donc pas trop éblouir par le prestige séduisant des études spéculatives; ne perdez pas de vue que c'est principalement dans le but de les appliquer à l'art de guérir que vous vous livrez à l'étude des hautes sciences." If this were always the case, we should not have the spirit of Mindererus, and spirit of hartshorn transformed from being more efficacious medicines, into purer chemical preparations, by the abstraction of oil of Dippel; a compound, which when prepared by the method of the older chemists, appears to have been (according to the researches of M. Klauer of Mulhausen, vide *Annalen der Pharmacie*, vol. xix. cah. 2, p. 135) eupione, holding in solution small quantities of picamare and capnomore, and owing to this admixture, its hot taste and agreeable odour. Another example is afforded of this departure from the true aim of pharmacy, in the neglect to which preparations made from the sulphuret of antimony have been subjected, since the discovery by Serullas of sulphuret of arsenic in this ore; although it is notorious that many old formulæ, in which this mineral was contained, enjoyed a high, and probably deserved, reputation in syphilitic and cutaneous affections. The sulphuret is certainly insoluble, but is it not a probability of importance, in the dearth of therapeutic means, that during prolonged ebullition, the sulphuret of arsenic, which it contains, may be converted into oxide, a remedy of great activity in these diseases.

Some there are who suppose that a series of legal enactments, similar to the edicts which prevail in many parts of continental Europe; laws for completely segregating the practice of pharmacy—for separating it from the practice of medicine—for regulating the number of pharmacies according to the population—for establishing a tariff by which the prices of medicines are to be arranged, &c.; that these laws are all that is necessary for making this department a kind of pharmaceutical El Dorado. We shall not now enter into any arguments for the purpose of showing the impracticability of this scheme, but merely refer its supporters to the comparative view of the state of pharmacy in Norway and the United States, which we have inserted among the "Medical Intelligence" of this Number, in which are contained facts sufficient to prove, how unnecessary legislative enactments are for the true interests of science. We shall merely in this place endeavour to demonstrate, that were the execution of this scheme as practicable and advantageous to the pecuniary interests of pharmaciens as its warmest admirers could desire, that it would be, notwithstanding, signally detrimental to the advancement of medical science.

Pharmacy, or the art of preparing and compounding medicines, is but a third portion of that great department of medical knowledge, "Pharmacologia." In the same category together with pharmacy, there are naturally combined the physiological action and the therapeutic use of medicines. Pharmacologia is intimately allied to many other sciences; it enters into direct relation with natural history, with chemistry, with physiology. But it is not any, nor all of these; it has for itself definite objects of pursuit; to determine the reaction of healthy and diseased living tissues, with external agents, and to ascertain the forms most fitting for their application to produce a certain effect: and these objects not being attained, we may be good chemists, good natural historians, or good physiologists, but are ignorant of pharmacologia.

Pitcairn proposed this problem, "a disease being given, to find its remedy." The terms of the problem are simple, its solution fraught with difficulties. It requires us to ascertain the essential difference of each disease; can morbid anatomy answer? It requires us to know the varieties which diseases may assume through the influence of inherited temperament; is physiology sufficiently advanced for this purpose? It requires us to measure the influence of accidents, such as climate, diet, complications, &c. And having learned thus much with regard to the disease, we must inquire in what does the active force of the medicine reside, whether in a mechanical or chemical property? what are the immediate results of its application to healthy tissues? what relation is there between its local and remote effects? what alterations are produced by differences of dose? what modifications are caused by disease? All these circumstances we have to take into account before the remedy is prescribed, and many of them must be kept in mind during its preparation. How necessarily connected then is pharmacologia with the other departments of medical science? What little reason had Xavier Bichat to denounce its fallacious processes, erroneous conclusions, and pompous puerilities, when those departments of practical medicine—to which it is naturally allied—were unable to afford it that assistance, without which the partnership is void.

The medical sciences do indeed constitute a partnership—not a sordid contract of pecuniary interests, but a partnership in all science, a partnership in all art, a partnership in all honour. Pathology, abstractedly, is a science distinct from therapeutics; and therapeutics from pharmacy: but in the practice of medicine they all unite. Loose this connexion by artificial means, and each will still form in itself a worthy subject of attention; but the great aim to which they all ought to be di-

rected will cease to be recognized ; no longer will the march of medical discovery resemble that of a disciplined army ; nay, the very success of isolated and independent detachments, may operate most injuriously on the remainder. Pharmacy, forming new alliances with chemistry and botany, may load your pharmacopœias with difficult and abstruse science, which will render them useless for those whose wants they were intended to supply.

But although we are opposed to the legislative separation of pharmacy from other departments of medical science, we are not the less anxious for the advancement of this art. The preparation and composition of medicine is essentially practical, and the only knowledge which can be of value, either for the exercise or enlargement of this department, is that which has been acquired by experience ; this proposition, which appears to be a truism, is, we fear, too much neglected by those to whom the education of our youth is committed.

True knowledge resides in experience alone ; the child may be told that virtue is sure of being rewarded, and vice of being punished ; but if not praised for his good actions, and blamed for his demerits, the lesson becomes useless, because not understood. It is very easy to read concerning the properties of minerals, plants, or animals ; and we may even make a very fair show in conversation by means of notions so acquired. The brilliant aldoboran may be invoked ; the dark-eyed gazelle may be admired ; the yellow-haired acacia may be eulogized by those who are unable practically to distinguish the great bear from the plough, the he-goat from an antelope, or papilionaceous flower from a butterfly.

The rage for book making, and for book reading, is the besetting sin of this age. The press groans with books of every size and price ; libraries accumulate in every city, and the demand equals the supply. We gladly sympathize with the love of knowledge thus displayed ; and if we deplore some of the evils to which this manner of exercising it may lead, we can easily understand why it is that reading is too generally preferred to careful observation and experiment. There is a volume which, we fear, is too much neglected ; and happy shall we be, if any effort on our parts may be in some degree successful in directing this longing after knowledge, from too exclusive a devotion to written pages, unto a greater reverence and closer scrutiny of the book of Nature.

Mankind are commonly under the influence of two very opposite motives—the love of ease, and the desire of fame. Both these feelings are capable of being gratified by the information which we acquire by reading. A few hours devoted to a methodical treatise, will afford us a much greater amount of apparent know-

ledge, than days spent in the dissecting room, the hospital, or the laboratory. We are inclined to glory in the rapid progress which we have thus made; we compare ourselves with others who have spent their lives in the practical study of Nature, and we find ourselves probably more fluent than they. We, perhaps, at some subsequent period, take up the scalpel, the stethoscope, or the crucible; but the time passes so slowly, there is so much patience and attention requisite; we learn so little thus, apparently, to what we acquired from books, that we soon become disgusted with our occupation, we forsake the practical study of our profession, and in some neighbouring library shortly become immersed in a systematic treatise. Reflecting on the frequency of this system, so fatal to the spread of actual knowledge, we can scarcely find in our hearts to execrate the fierce Saracen who devoted the Ptolomeian library to the flames.

The only remedy which we can propose for this evil in our profession, is the establishment of practical examinations, by those whose duty it is to preside over the progress of medical literature. It is no imaginary ill which we desire to remove. In the departments of medicine and surgery, its effects are not so remarkable as in pharmacy; a certain amount of practical knowledge is so palpably indispensable for success, that the student is forced, by an innate sense of right, to attend to the hospital and dissecting room. But with pharmacy the case is different; we have known chemical processes to have been glibly described by those who had never handled a retort; we have heard the characters of many Jusseuan families repeated with parrot-like facility, by gentlemen who were incapable of distinguishing, by means of a *Flora*, a red rose from a red cabbage. But this reproach does not merely apply to the students of these kingdoms; among the much vaunted *élèves* of the School of Pharmacy in Paris, we find the same canker. At the annual concours on Toxicology, held on the 12th of December, 1838, the candidates were subjected to three tests, viz. a verbal examination, a written examination, and a proof of actual manipulation. With respect to the first, the reporter (M. Soubeiran) states, that the answers of the candidates were generally very satisfactory; one in particular excited general attention from the complete manner in which he embraced his subject. With regard to the written test, three compositions were considered very excellent, the questions were treated in a very masterly manner, and a few slight errors by no means obscured the real value of the essays. But when it came to the practical test, and the candidates were required to ascertain the presence of arseniate of soda in wine, of sulphate of zinc in soup, and of sulphuric acid in an albumi-

nous fluid, among six gentlemen, two could not discover the arseniate of soda; another detected morphia in a solution of zinc, and tartaric acid in the solution of sulphuric acid; another found brucia in the liquor which contained arseniate of soda, tartar emetic in the acid liquor, and a mixture of tin and zinc where the latter was alone present. “*L’ecole*,” says the reporter, “*a vu avec peine ce candidat qui s’était fait remarquer dans l’épreuve verbale, descendre ainsi du rang qu’il s’était si justement acquis.*”

We ask is this knowledge? no; it is an ignorance the most profound, infinitely more so than that of the peasant, who, destitute of letters, is equally devoid of parrot-like complacency and self-conceit. It is a truth which can never be too deeply impressed on those who have the regulation of professional education, that the love of learning, the eager desire of practically investigating, is infinitely of greater importance, in the onward march of science, than the mere possession of information. He knows not the pleasure of the chase, who employs others to bring down the prey. We conclude, that it is not by the absurd dogmatism of legislative interference, instituting artificial barriers, doomed to perpetual infringement, and the subjects of eternal litigation, that a knowledge of pharmacy is to be encouraged or diffused; on the contrary, we are quite sure that if its practical study be promoted by the establishment of prizes, and by examinations adapted for that purpose, no violent revolution with respect to the present condition of the Profession is necessary for its advance.

We have been led into these reflections by the perusal of the work whose title stands at the head of this article. It was to be expected that a pharmacopœia constructed by the distinguished physicians who constitute the Edinburgh College, should eminently fulfil the intentions of this important publication; with a fame too well established to need any unnecessary display of pedantic knowledge, those gentlemen were at liberty to devote their energies to the composition of a work calculated to supply the every day wants of the Profession. These expectations have been in a great measure fulfilled; and in studying the work before us, we know not, whether we are most pleased by the fidelity with which the march of useful pharmaceutical knowledge has been tracked, or by the modesty and simplicity with which unavoidable imperfection has been acknowledged.

We shall, in the remainder of this article, bring before our readers some of the novelties with which the new Edinburgh Pharmacopœia abounds; and in lieu of entering upon lengthened criticisms, we shall endeavour to relieve the dryness, which must

otherwise appertain to a notice of this description, by appending to each subject the most interesting pharmaceutical intelligence, gleaned from the later foreign journals.

Our readers are most probably already aware, that this work is written in English. Into the arguments for and against this change we shall not now enter, but merely remark that it has the advantage of enabling the compilers to give directions sufficiently explicit for each preparation ; a desideratum too long overlooked by the framers of pharmacopœias. The nomenclature adopted is as much as possible in unison with the names most generally known ; this is just as it should be, for we agree with the sentiment expressed in the preface, that “the more decorous dress of science or philosophy has been dearly purchased at the cost of being compelled to follow the changing fashion of the day.”

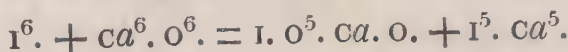
To enable practitioners to defend themselves and the public against the too common practices of sophistication, short notes are added to the principal drugs placed in the list of *materia medica*, by which their purity can to a certain extent be ascertained. A similar endeavour on the part of the framers of the London Pharmacopœia, was signally abortive. The reason of this want of success was, that in the latter case, the great object kept in view was the discovery of impurities. Now in the greater number of cases, this is what the practitioner least desires to learn. What is it to him if sulphuric acid should contain a small quantity of sulphate of lead ? or if nitre should contain a minute proportion of chloride of sodium ? Most usually, what he wants to know is, whether the medicine he exhibits contains a sufficient quantity of active ingredients ; and this want has been most admirably supplied in the Edinburgh Pharmacopœia. We shall give a few examples.

“ *Cinchona Flava*. Yellow bark ; from an unascertained species of *Cinchona*. A filtered decoction of 100 grains in two fluid ounces of distilled water gives, with a fluid ounce of concentrated solution of carbonate of soda, a precipitate, which when heated in the fluid becomes a fused mass, weighing when cold two grains or more, and easily soluble in solution of oxalic acid.”

It will be at once perceived, that this test aims at the quantity of alkaloid, which is expected to be contained in the given quantity of bark. It has lately been ascertained by Dr. Langlois of Strasbourg, that carbonic acid will not unite with cinchonia and quinia ; the precipitates thrown down from sulphates of these bases, by alkaline carbonates, being the pure alkaloids, which only effervesce with the stronger acids, in the ratio of the quantity of alkaline carbonates which may happen to be mixed with them.

“*Iodineum.* Iodine. Entirely vaporizable: thirty-nine grains with nine grains of quicklime and three ounces of water, when heated short of ebullition, slowly form a perfect solution, which is yellowish or brownish if the iodine be pure, but colourless if there be above two per cent. of water or other impurity.”

This test demonstrates the hand of a master; when iodine is boiled with lime, iodate of lime and iodide of calcium are formed, according to the following formula—



Now the atomic weight of lime being 28,528, and that of iodine 126,567, it follows that 9 grains of lime will require 39,929 grains of iodine; but the lime, always containing some impurity, is never in practice capable of uniting with so much, and thus some iodine is left free, which, dissolved by the iodide of calcium, colours the liquid.

“*Opium.* Concrete juice from the unripe capsules of *Papaver Somniferum*. A solution from 100 grains, macerated twenty-four hours in two fluid ounces of water, filtered, and strongly squeezed in a cloth, if precipitated by a cold solution of half an ounce of carbonate of soda in two waters, and heated till the precipitate shrinks and fuses, will yield a solid mass on cooling, which weighs, when dry, at least eleven grains; and if pulverized, dissolves entirely in solution of oxalic acid.”

This test ought always to be used with every fresh purchase of opium, inasmuch as this drug has latterly been very much adulterated; the foreign journals contain many instances of this fraud, some specimens not containing two per cent. of morphia, and evidently mixed with the extract of the capsules. These manufactured articles may be distinguished also by wanting, in the fresh section or fracture, the yellowish colour of real Turkey opium, but on the contrary, presenting the reddish-brown appearance which is the result of exposure to the air.

“*Crotonis Oleum.* Expressed oil of the seeds of *Croton Tiglium*. When agitated with its own volume of pure alcohol, and gently heated, it separates on standing, without having undergone any apparent diminution.”

The foregoing examples will sufficiently show the care which has been bestowed on this department. In other instances, when necessary, tests are recommended for demonstrating the presence of impurities; these we need not dwell on. We may remark what little dogmatism is displayed by the learned authors of this work, in deciding on the plants from whence drugs are obtained. For example, the different kinds of Aloe in commerce are said to be the—

“Inspissated juices of various species of Aloe, not yet accurately determined.” “*Copaiba*. Fluid resinous exudation of various species of *Copaifera*.” “*Euphorbium*. Concrete resinous juice of undetermined species of *Euphorbia*.” “*Manna*. Sweet concrete exudation, probably from several species of *Fraxinus* and *Ornus*.”

How much more philosophical is this conduct, than the attempt to decide, when data are insufficient.

We shall now direct our attention to the “preparations and compositions.” The process for preparing acetic acid is similar to that which we recommended in our review of the London Pharmacopœia. We shall quote the process as a good example of the accuracy and care with which the necessary directions are commonly given in this work.

“*Acidum Aceticum*. Take of acetate of lead any convenient quantity; heat it gradually in a porcelain basin by means of a bath of oil, or fusible metal, (8 tin, 4 lead, 3 bismuth,) to 320° F. and stir till the fused mass concretes again; pulverize this when cold, and heat the powder again to 320° , with frequent stirring, till the particles cease to accrete. Add six ounces of the powder to nine fluid drachms and a-half of pure sulphuric acid, contained in a glass matrass; attach a proper tube and refrigeratory; and distil from a fusible-metal-bath, with a heat of 320° , to dryness. Agitate the distilled liquid with a grain or two of red oxide of lead, to remove a little sulphurous acid, allow the vessel to rest a few minutes, pour off the clear liquor, and redistil it. The density should not be above 1065.”

“I cannot conceive,” said the late Dr. Wollaston, on a certain occasion, “what advantage you can propose to yourself by using oil as a heating agent. If you wish to obtain a temperature of 212° , use boiling water; or a temperature of 600° , use boiling mercury; and if you desire to set the house on fire, use boiling oil.”

A curious fact in the history of acetic acid, is the difference which exists between the point of greatest concentration, and that of greatest density; the composition of the former, according to Berzelius, being $\overline{Ac} + HO$, that of the latter $\overline{Ac} + {}^3(HO)$. A great difference of opinion seems to prevail as to the greatest density; Mollerat stating it to be 1,079, while M. Persoz places it, in a late essay, at 1,12. It will be seen that an acid of greater density than what the Pharmacopœia states, will be consequently weaker.

Leibig has lately, in a great measure, decided the theory of acetification; he demonstrates that alcohol ($C^4 H^5 O + HO$) differs from Aldehyd ($C^4 H^3 O + HO$) in containing two additional atoms of hydrogen, while ($C^4 H^3 O^3 + HO$) is the formula for acetic acid. He therefore presumes, that the oxygen of the air first removes

two atoms of hydrogen from alcohol, forming aldehyd, and then affords two atoms of oxygen to the latter, forming acetic acid. He has obtained, however, practically, only $\frac{1}{5}$ ths of the quantity indicated by this theory.

The process recommended in this pharmacopœia for obtaining benzoic acid, is similar to the London, viz. by direct distillation. Mohr, in the *Annalen der Pharmacie*, vol. xxxix. cah. 2, p. 178, has written a valuable paper on this preparation. He recommends a vessel eight or nine inches in diameter, and two inches deep, the bottom of which is to be covered with about a pound of coarsely powdered benzoin; the top is then to be covered with spongy unsized paper, pasted on the rim, and over that is to be fastened tightly with twine, a thick, well-sized paper cap, in shape somewhat like a man's hat. The heat is to be applied gradually by means of a hot-hearth. The advantages of this process consist in the purity of the product—the flowers of benzoin being forced to sublime through the pores of the unsized paper diaphragm, and thus being freed from oil and colouring matter; the sublimed matter is prevented from falling back into the fused balsam; and it is also protected from the heat radiating from the bottom of the vessel.

Hydrocyanic acid is directed to be prepared by the same method as in the London Pharmacopœia. M. Ossian Henry has published some very interesting researches for demonstrating the presence of very minute proportions of this acid. His method consists in distilling the suspected liquid into a receiver containing a solution of nitrate of silver, until a film forms on the latter and precipitates; he then heats this precipitate of cyanide of silver gently, with half its weight of chloride of sodium; (the weight of course must be guessed;) cyanide of sodium in solution, and insoluble chloride of silver are the results. He separates the latter by filtration, and adds to the liquid a little freshly precipitated green oxide of iron; a portion of the iron becomes converted into cyanide, which uniting with the cyanide of sodium, forms ferrocyanide of sodium, the solution of which he purifies by another filtration; and then to separate portions adds his tests. Perchloride of iron throws down a blue, and sulphate of copper a brown precipitate. The delicacy and accuracy of this method of operating may be judged from the fact, that nitrate of silver will precipitate with a liquid containing $\frac{1}{40000}$ of prussic acid; and perchloride of iron will produce a perceptible blue colour, with a solution holding $\frac{1}{40000}$ of ferrocyanide of potassium. As an accessory proof, he recommends the conversion of a portion of the cyanide of silver into sulphocyanide, by heating it with an equal quantity of sulphur, fusing, and triturating the residue with chloride of sodium, dis-

solving, filtering and testing, with perchloride of iron, (a red solution,) or sulphate of copper, (a white precipitate).

As there is not any thing of sufficient importance to detain us among the acids, we shall proceed to the next section, "Alcohol and Ethers." The process for rectifying alcohol consists in permitting lime to slake in rectified spirits; and when the slaking is finished, distilling with a gradually increasing heat. The density of the alcohol so obtained, is stated to be 796: "if higher, the distillation must have been begun before the slaking of the lime was finished." M. Soubeiran recommends the spirit to have been previously digested on carbonate of potash; under those circumstances he found the first and last portions of the distilled liquid to contain water, but the middle product was absolute alcohol. He also ascertained that the alcohol acquired no disagreeable flavour from the lime, as some have stated. Using lime there is a great loss of alcohol. M. Liebig attributes this to the decomposition of alcohol by the lime; according to his theoretical opinions, alcohol is a hydrate of ether, in which the water plays the part of an acid; but potash and lime being stronger bases than ether, rob the latter of its acid, (water,) by the force of superior affinity: the ether thus liberated, enters into combination with another portion of the stronger base. He appeals, in evidence of this operation of affinities, to the phenomena which take place in the purification of potash by alcohol; when we evaporate a solution of potash in alcohol to dryness, and increase the heat, the mass swells, blackens, and liberates inflammable gases; this he ascribes to the decomposition of ether, which had been previously combined with the potash. M. Soubeiran does not subscribe to this opinion; he finds that chloride of calcium also retains a large quantity of alcohol; he is inclined to believe that the alcohol is mechanically retained by the porosity of the lime. In the present state of the question, we prefer M. Liebig's explanation.

The process for preparing sulphuric ether differs from that usually practised; thus the spirit is desired to be added to the acid, instead of the acid to the spirit, and the materials are to be briskly agitated, previously to pouring them into the matrass; the quantities employed are also new, fifty ounces of spirit to ten of sulphuric acid; twelve of spirit being first used, and the remainder added, by a described apparatus, during the distillation of the ether. For rectifying the distilled liquid, a saturated solution of muriate of lime, containing some slaked lime is ordered; and after the complete disappearance of sulphurous acid odour, the decanted ether is to be distilled as long as it maintains a density of 735. We cannot judge of the comparative merits of this process, not having practised it; but we

can answer from experience for the excellence of the refrigeratory recommended in the preface.

“ This refrigeratory consists of a long, narrow cylinder, slightly inclined to the horizon, and of a tube which passes along the centre of the cylinder, and is fixed at each end, so that the space between them is air-tight; and by means of a funnel entering at the lower end of this inter-space, and an exit tube from its upper extremity, a stream of cold water may be kept constantly running, by which refrigeration and the condensation of vapours within the inner tube are far more effectually accomplished than by any other mode that has hitherto been devised.”

We are surprised that no preparation analogous to the “compound spirit of sulphuric ether” of the London Pharmacopœia, has been introduced into the present work. We find nothing to delay on in the “spirit of Nitric Ether.” We may here remark that Professor Hare of Philadelphia has lately discovered an exceedingly volatile and curious ether, under very peculiar circumstances. Every one knows that nitric ether, as it was accustomed to be called, is a compound of hyponitrous acid and ether; and that during its formation the nitric acid has to part with two atoms of oxygen, which uniting with the elements of a portion of alcohol, causes the production of certain products, together with a loss of materials. Professor Hare imagined that if a hyponitrite was decomposed by sulphuric acid, in contact with alcohol, this loss might be avoided. By putting this hypothesis into operation, he obtained an ether totally dissimilar from that previously known, having a different taste and smell, entering into ebullition at 65° F., and producing by spontaneous evaporation a cold of 15° below zero. Touched by the finger, this ether hissed, like water touched by a red-hot iron.

We cannot leave the subject of the ethers without referring to a theory of their formation and composition, first promulgated in this Journal by Dr. Kane, and almost simultaneously announced by Liebig. Having ascertained that all the ethers of the oxacids contained the elements of water, while those of the hydracids did not, Dr. Kane was struck by the analogy thus presented to the salts of ammonia; and inasmuch as the progress of science had determined, that the oxysalts of this latter base, possessed for their radical oxide of ammonium, ($O + N. H^4.$), and not water with ammonia ($HO + NH^3$) as was formerly supposed; and that the salts of the hydracids are composed of an electro-negative element, combined with ammonium, ($N. H^4.$), and not of an hydracid united with ammonia ($N. H^3.$) (the old hypothesis,) he was led to apply similar views to etherial combinations. The theories of the ethers previously entertained were, first, that of

Thenard, who held that olefiant gas ($c^2 H^2$) was the radical of the ethers, and that therefore sulphuric ether was expressed by the formula, $H. O. + 2 (c^2 H^2)$; muriatic ether, $H. ch. + 2 (c^2. H^2)$, and hyponitrous ether, $N. O.^3 + H. O. + 2 (c^2. H^2)$; these being the exemplars of three great groups: and the hypothesis of Dumas and Boullay, who considered etherine, $c^4. H^4$. to be the radical of the ethers; but, we think, without a single tenable argument in their favour. Drawing on the analogy of ammonium, Dr. Kane conceived that the base of the ethers must be a radical composed of $c^4. H^5$., to which he gave the name of etherium; and that, consequently, sulphuric ether should be expressed by the formula, $O + c^4. H^5$., muriatic ether by $ch + c^4. H^5$., and hyponitrous ether by $NO^3 + O + c^4. H^5$.

Liebig, in the *Annalen der Pharmacie*, vol. xxiii. cah, 1, p. 12, argues powerfully in favour of this view. "Are oxygenized organic matters," he asks, "the oxides of compound radicals, or are they the combinations of radicals with compound bodies? Are they the oxides or hydrates of carburetted hydrogen? This is an important question, and of a great influence on all the ulterior researches which we have presently to consider."

"In aldehyd we have the hydrate of an oxide, $c^4. H^3. O. + H. O.$ capable of combining itself again, in two proportions, with oxygen; an oxide feebly acid; another strongly so.

"In alcohol we have the hydrate of another body, capable of neutralizing acids, and of forming with them combinations, in which we find all the proportional relations of the compounds of inorganic oxides. Ether can act as an oxide; it can act as a base: we can replace the oxygen of ether in alcohol by sulphur, the water in this hydrate by another combination of hydrogen, by sulphuretted hydrogen: we can replace the hydrogen of the latter by a metal. All those facts reunited lead us to this point, that one atom of hydrogen, and one atom of oxygen, are contained in alcohol under another form than the remaining five atoms of hydrogen, and one of oxygen. We conclude that the two former exist under the form of water, for we can with ether and water reproduce alcohol. These conclusions appertain to both theories: they repose on undeniable facts."

M. Liebig then proceeds to rebut the arguments which have been adduced against the etherium theory, principally, we believe by M. Dumas, in the fifth volume of his *Chemistry applied to the Arts*. It has been asked, why ether has not an alkaline reaction? by those who admit that olefiant gas is a base: why ether does not combine with acids by simple mixture? by those who are aware that lime will not unite with sulphuric or carbonic acids unless it be in the state of hydrate: why neutral salts of the ethers will not exchange principles with other neutral salts? The answer is, that ether differs from potash and

ammonia, precisely because it is ether, and not potash or ammonia. It would be surely laughable to question the existence of sulphate of platinum, because alkalies will not precipitate its oxide. Sulphuric acid will not precipitate from sulphovinic acid, with baryta; nor will oxalic acid from oxalovinic acid: the latter acid will form a soluble salt with lime; and chlorine cannot be detected in muriatic ether by nitrate of silver; for the same reason, without contradiction, that cyanuret of silver is not decomposed by hydrate of potash; the cyanate of silver by metallic chlorides; nor fulminate of silver by hydrochloric acid.

These are powerful arguments; but what is the legitimate deduction to be drawn from them? Simply, that ether is a base; that no line of demarcation can be drawn between it and inorganic bases; that those compounds which have been called organic, are produced by, and are obedient to the ordinary play of chemical affinities. It may proceed from our want of capacity, but we confess that we cannot perceive in the foregoing arguments, any thing which would incline us to decide that the ethers are not hydrates, but the oxides of a radical.

Oxalic acid, says M. Leibig, may be decomposed by sulphuric acid, into carbonic acid and carbonic oxide: formic acid, by the same, into water and carbonic oxide: yet the supporters of the etherine theory (or of the olefiant gas theory) do not deduce from these facts the composition of oxalic and formic acids; but with respect to ether they are less scrupulous.

Well, we may be permitted to say, this only proves the inconsistency of these gentlemen; it is no evidence whatever of the truth of the etherium theory.

M. Liebig denies that alcohol is decomposed during the formation of ether, by the subtraction of water; on the contrary he asserts, that in every instance, with the exception of fluoboric ether, the acid combines with the ether and not with the water. This assertion, however, must be qualified; in the formation of sulphovinic acid, the sulphuric acid unites with both ether and water. And, again, is it perfectly decided that a vinic acid is a necessary preliminary in the formation of ether? M. Guerin says, that tartro-vinic acid, when subjected to heat, yields only a little acetic ether. If sulphovinic acid be the substance from whence sulphuric ether arises by decomposition, why is it that a temperature between 260° and 320° F., is required for the formation of this ether, when the acid in question decomposes at a low temperature, and sometimes spontaneously? Again, how do the supporters of this doctrine account for the formation of ether by chloride of zinc? M. Masson has found, that alcohol treated with hydrated chloride of zinc, becomes transformed into ether and water at 266° F. gives off sweet oil of wine ($C^4.H^4$) at 311° or 320°, as far as 428°; and according as the latter in-

creases, so does the water, the quantity of ether diminishing. During the whole process great quantities of carburetted hydrogen are liberated. The hydrated chloride of zinc remains, partly converted into oxide; hydrochloric acid being liberated with the volatile products. We cannot reconcile these facts with the manner in which M. Liebig supposes ether to be formed.

“It cannot be indifferent,” says M. Liebig, “whether we give a preference to a fiction, or to observations well founded. It is contrary to reason to admit in ether, water and olefiant gas, when the existence of one or the other cannot be demonstrated by a single fact. We do not discuss explanations, but principles: the theory of oxygenized muriatic acid explained every thing, and yet we have abandoned it.”

It appears to us, that the theory of oxygenized muriatic acid was abandoned, for the very reason, why we should be cautious in receiving the etherium theory. Oxymuriatic acid was forsaken, because its existence could not be demonstrated by experiment. Has any one ever isolated etherium? we know that olefiant gas does exist.

The analogy between the ammonium and etherium theories is equally defective. Why was the ammonium theory adopted? because there was sufficient evidence of there being such a compound; ammonia and hydrogen when mixed, under certain conditions, possessing the property of amalgamating with mercury, which neither could separately. And secondly, because the compounds of potassium, and this ammonium, are isomorphous. A glaring exception was thus brought within the limits of general laws.

But it is otherwise with the etherium theory; if you accept it you must totally change the face of organic chemistry. You must unite every alcaloid with hydrogen, and create innumerable imaginary radicals: this, indeed, M. Liebig has been consistent in effecting. In a similar manner, Dr. Kane has, in his *Essay on Pyroacetic Spirit*, created a host of these æreal beings. The reasons ought to be powerful which can justify such sweeping alterations.

One argument appears to have been overlooked by the supporters of this theory; if sulphovinic acid be a double sulphate of ether and water, and that ether be an oxide analogous to potash, the latter ought to be capable of displacing the ether in this compound, if, as M. Liebig states, the law be true that one body can only be displaced by another of the same class, following the measure of its affinity. We ask, why does it not do so? why should the water be displaced in preference?

To recapitulate; the etherium theory requires us to suppose the existence of a compound which has not hitherto been

demonstrated : the olefiant gas theory does not require any such stretch of imagination. The etherium theory does not account for the liberation of olefiant gas under all circumstances : the olefiant gas theory does. The etherium theory requires us to produce a total revolution in organic chemistry : the olefiant gas theory does not. Both theories equally well explain the greater number of phenomena ; but it is principles, not explanations, we are desired to discuss. Which theory shall we prefer ?

We must adjourn the further examination of the Edinburgh Pharmacopœia until the next Number.

A Lecture on the Nature, Treatment, and Cure of Club-Foot ; illustrated by Cases. By M. H. STAPLETON, M.B., T.C.D., Licentiate of the Royal College of Surgeons in Ireland, and Surgeon to Jervis-street Hospital.

WE have read with pleasure this lecture. It contains within a few pages a concise, and yet a sufficiently full account of one of the greatest advantages which surgery has conferred on society, the cure of the unsightly deformity of club-foot. The author divides the cases of club-foot into two classes ; the first congenital, and the second arising from accident ; and cases of recovery from both classes are given in the essay.

The history of the operation is traced up from the first attempts of the early operators, in which the tendon, skin, and cellular tissue were all divided in the one incision, and a large, gaping, and dangerous wound left, through Delpech's improvement, in which, however, suppuration was allowed to take place ; and finally to the present (we may say perfect) state of the operation, which may be described as both bloodless and painless. The author has adopted Duval's improvement, which consists in introducing the tenostome on the inside of the leg in varus, by which the inner edge of the tendon is more certainly divided. He does not recommend the performance of the operation at too early an age.

“The best period is from three months to a year old. Younger than this the skin of the infant is remarkably delicate, and is not sufficiently hardened by the contact of the air ; the limbs are also so short that there is not sufficient room for the application of an apparatus ; moreover infants before this period are in the habit of keeping their legs flexed, and are much annoyed by the straight position ; add to this the constant necessity for changing the bandages. Infants at this period suffer much from the derangement of their bowels, and the constant crying which this occasions, in-

stead of being attributed to its proper cause, will be said to be occasioned by the means which you have adopted for the cure of the malformation of the foot."

The apparatus which the author employs, it is scarcely necessary to observe, is the boards of Stromeyer. Six cases are given, in all of which the treatment has been completely successful. In only one case an interval of ten days was allowed to elapse after the operation, before the apparatus was applied. In all the other cases, the apparatus was applied at the end of three days from the time of the operation; and the average duration of time occupied in the cure was only four weeks.

SCIENTIFIC INTELLIGENCE.

Remarks on the Epizootic Epidemic. Translated from the French
by GEORGE WATSON, M.D., M.R.C.S., &c.

[Continued from No. XLVI. p. 77.]

ROYAL ACADEMY OF SCIENCES.—*Sitting of the 18th of Feb. 1839.*

Milk of Cows affected with Cocote.—M. Chevreul commenced, in the name of the Commission, to which M. Turpin had been joined, the reading of a report on a note presented by M. Donne, on the subject of the alteration which the milk of cows attacked by the cocote presents. This report comprises the *exposé* of the note in question, the researches proper for the Commission of the Academy, the determination of the effects of this milk upon the health, and the indication of duties to be undertaken in this department in case of an epidemic.* It may be remembered, that, according to M. Donne, in the beginning of the disease amongst the cows, when there were only some pustules on the dugs, the milk, observed by the microscope, did not present any change: the second day, the characters of the colostrum of women are discovered in it, to wit, acidity, the presence of four sorts of globules, butyraceous, agglomerated, muriform, and mucous, and lastly, the property of becoming viscid by its mixture with ammonia.

These effects became more and more prominent in proportion to the progress of the malady; and on the tenth day, when the dug was attacked by the engorgement which the dairies designate "*cru*," the milk obtained was mingled with pus, greenish, foetid, offering both the odour of the acids of butter, and of putrid animal matters. The neighbouring dugs might, moreover, furnish a milk of good quality, if the disease had not extended to them. The Commission has desired to verify these results; it procured ten samples of the milk of affected cows, five coming from the farm at Grignon,

* If I may be allowed to say so, it is especially for the sake of this sketching out of the duties and scope of that important application of medicine, medical police, connected with the immediate subject of interest, which is my excuse for obtruding a copiousness of extract, and in a few points a trifling repetition of matter, on the pages of this Journal; feeling certain that these subjects, in which our continental brethren and their governments have taken the lead, must meet with fuller attention from our practitioners and constituted authorities; the pressing of which upon the medical public should need neither apology nor recommendation.—Tr.

and the other draughts, obtained in the presence of the Commission in the slaughter-house of Montmartre. Although the quantities of milk were insufficient to make suitable analytic researches, the Commission has recognized the existence of four kinds of globules, and the inspissation by means of ammonia.

As to the acidity, it was not general; many samples presented an alkaline reaction; in three cases the different teats gave different milks. The reporter here takes notice of a curious observation due to M. Robiquet. When the milk becomes viscid with ammonia, the addition of acetic acid, instead of causing therein a precipitate, only occasions a slight trouble. The same observer has also recognized that the milk filtered is no longer inspissable by the alkali, which proves that the matter which gives it this property is only in suspension.

M. Lassaigne has made, upon the milk of the cows of Alfort, observations analogous to those of the Commission; he has besides recognized that a cow sick, but not with "cocote," yielded a troubled milk which coagulated spontaneously: he attributes this property to the presence of albumen. "En resumè," the milk of cows attacked with "cocote," would have for characters deficiency of homogeneity, and of liquidity, the presence of globules of different kinds, and the inspissation by means of ammonia. But it most imports us to establish, that this milk cannot have any injurious action on the economy; a proposition confirmed moreover by chemical and microscopic analysis, since it presents the constitution of the *colostrum*, and that the special matters found in some rare specimens, do not appear to be any thing else but fibrine or albumen, substances, as we know not only innoxious, but endowed with even nutritive qualities in a high degree.

The second part of the report of M. Chevreul has relation to the researches that it would be suitable to undertake in the cases of epizootic epidemic, or contagious disease, in order to render available the light furnished by chemical analysis. These researches should bear upon the organic matters and upon external agents, such as the air, water, &c. As to what refers to organic matters, we comprehend without difficulty that it is necessary to enlist as aids the data furnished by medicine, in order to determine both the symptoms, the reunion of which constitutes the disease, which it is needful to study in the morbid products it gives rise to, and the lesions of tissues which accompany these symptoms: hence also, the necessity of exact analysis of the fluids or of the tissues considered in a sound state, in order to make use of them as points of comparison; unfortunately, in the actual state of science, these analyses are completely wanting. Still more also, the proportion of the immediate principles of the blood, milk, &c. is not reduced to formula sufficiently precise in order to our executing of them; and these principles themselves do not always present characters sufficiently positive to render it possible to discriminate them from each other: thus caseum, fibrine, concrete albumen, submitted to the action of chemical agents, offer properties almost identical. What must be the result then, if to the causes of error we join fur-

ther those which are inherent in the paucity of the proportion of the heterogenous principle, engendered in a humour or a tissue by disease, and in its transient existence, which may prevent the verification of the inductions furnished by a first examination.

The analyses then offer an excessive difficulty: they will be without doubt rendered more accessible to the efforts of chemists by the progress of science. Thus before the labours, so remarkable, of M. Chevreul himself upon fatty bodies, it would have been impossible to characterize the fats contained in the blood or bile, whilst at the present day the results obtained on this subject are most satisfactory.*

The microscopic researches are not of less importance. The animal fluids in spite of an apparent homogeneity, are often constituted by globules, which swim in a liquid transparent or opaque; such are the blood, milk, pus, &c. However, we should not lose sight of the fact in this case, that identity of form does not prove that of nature; it suffices even that this form be simple in order that it may apply itself to substances very different. Identity of chemical characters can alone throw light upon the true nature of bodies that we may be studying.

If we make the application of the principles which have just been set forth to milk, we may see that the normal analysis of this liquid remains yet to be accomplished; that in butter, for instance, the proportion of fatty acids is far from being fixed in a precise manner; that caseum would need to be studied in a more complete manner, comparatively to fibrine and to coagulated albumen, in order to establish the alterations which it is susceptible of undergoing under the influence of diseases. The most simple reactions of milk form still a subject of discrepancy among observers, this liquid being acid according to some, alkaline according to others. The Commission, desirous of having, upon this point, some positive data, has prosecuted at Alport some experiments, whence it results that milk directed upon the reacting paper, on its exit from the teat, is habitually, but not always, alkaline. We may recall on this occasion what M. Lassaigne has observed in cows, the milk of which offered, after some days' interval, opposite reactions on vegetable colours.

The presence of pus in the blood, or in the milk, is it more easy to establish? Every chemist will reply in the negative; for it is not sufficient to ascertain therein the presence of globules of a particular form, or to shew that the liquid is inspissated by ammonia, the globules of mucus and of pus having the same characters, and the colostrum, which has nothing morbid in it, becoming also viscous by the addition of alkaline, as M. Donné has himself shewn: here then it is necessary to return to the study of immediate principles.

* This eulogy on himself by M. Chevreul, in this report, is, to say the least, rather remarkable, and very *French*.

The researches which it would be desirable to undertake upon external agents, give place to considerations analogous to those we have just alluded to. Up to the present no result has been arrived at; it is not because one has not recognized in the air principles foreign to its ordinary constitution. We have been enabled by gathering dew condensed on the sides of a vessel filled with ice, therein to demonstrate the presence of organic substances; an hydrogenated element in the air has been equally put out of doubt as to its existence; but it might be necessary to prove that these principles were endowed with noxious properties, and this demonstration is so much the more urgent, inasmuch as, on the one part, our efforts of industry and a host of other acts, natural or artificial, diffuse into the air elements of organic origin, and that, on the other hand, the presence of a new immediate principle in a focus of disease may be, not the cause, but rather a symptom, of the prevailing affection. In opposition to these considerations, we cannot deny the existence of a deleterious agent, in relying upon what one has not proved it to be: how many times have not metals been mistaken which were hid in small portions in alloys? It would be requisite then, by the aid of mechanical means, as compression or cooling, to seek to condense the miasmata contained in the air. We should also endeavour to separate them from the solid or liquid matters where they escape, or even to determine the formation of them; since we should study them by the ordinary chemical processes. The examination of waters destined for domestic uses, gives rise to considerations of a like nature, and so much the more important, because certain arts have for effect, the vitiation of the waters they employ, by introducing therein poisonous matters, such as the salts of arsenic. We may conceive then the danger which might result from the mixture of these waters thus altered, with those which we intend to serve as drink.

Extract from the Report made to the Academy of Sciences upon the Milk of Cows affected with the Cocote, followed by general Considerations relative to the Search for active Matters in the Animal Economy, which may be found in morbid Products, in the Atmosphere, and in the Waters.—Milk has been, in these latter times, the object of numerous researches, which have thrown light upon some points of its chemical history. Some of these labours have been undertaken on the occasion of the epizootic, which, under the name of *cocote*, has prevailed latterly among the cows in the suburbs of Paris; and all have received, from this circumstance, a fresh degree of interest, which renders it a duty incumbent on us to present the *resumé* to our readers.

We do not think that we can do better, in order to fulfil our object, than by giving an extract of the report made upon this subject to the Academy of Sciences, by M. Chevreul, on behalf of the chemical section. This report had more particularly for its object the labours of M. Donné; but the learned critic, extending the scope of the question, has attached to it considerations of the high-

est importance, upon the applications of chemistry to the study of active matters in the animal economy which may be found in the morbid products, the atmosphere, and the waters. He has established the general principles which should direct chemists in researches of this kind. This second part, which its extent prevents us from giving at length, is in some sort a complement to the treatise of organic analysis, published by the author. In examining, in the first place, the labours of M. Donné anterior to the appearance of the *cocote*, M. Chevreul remarks:

“ M. Donné acknowledges, with Leuwenhoëck, and all those who since this observer have submitted milk to the microscope, that this liquid is composed of two distinct parts: first, water holding in solution different bodies; second, globules which, in suspension in this water, give to it the milky aspect. But M. Donné only admits of the fatty or butyraceous globules, whilst Leuwenhoëck also admits therein globules of another nature. M. Donné founds his opinion on the following experiments.

“ 1st. The milk, when filtered, leaves upon the paper butyraceous globule soluble in ether, and the filtered watery liquid is transparent.

“ 2nd. When we treat milk, upon the object glass of the microscope, by ether, we perceive the globules dissolve in this latter.

“ M. Donné only admits in milk, in a normal state, butyraceous spherical globules of a variable diameter, 1.500 to 1.100 of a millimeter, and even beyond this. To their chemical character, of being soluble in ether, it is necessary to add that of not being soluble in potass, except after having been saponified.

“ M. Donné, agreeing with M. Dujardin, and in opposition to M. Raspail and M. Turpin, does not recognize any appearance of organic structure in these globules; he does not consider them then as formed by a membrane or cellular tissue enclosing butyraceous matter, but rather as small spheroids resulting from butyraceous particles united by the force of cohesion.

“ The milk of woman, which shews itself immediately after delivery, is very distinct from normal milk, that is to say, from that of a good nurse, to date from the second month after confinement, on which account it has been distinguished from this latter by the designation of *colostrum*, and microscopic examination fully justifies this distinction. In fact normal milk only presents, on examination, isolated globules, moving themselves about in the bosom of a clear liquid by the least agitation; whilst M. Donné has observed, in the *colostrum*, as many as four sorts of globular matters.

“ 1st. Isolated butyraceous globules, like those of normal milk.

“ 2nd. Small butyraceous globules, bound together by a viscid matter.

“ 3rd. Muriform globules, that is to say globular agglomerations, having the form of mulberry fruit.

" 4th. Mucous globules, similar to those of the mucus of the mouth, nostrils, &c.

" In short, M. Donné has assured himself, that ammonia mixed with milk does not change the consistence of it, whilst its mixture with the colostrum yields a viscid liquid, which can even assume a jelly form. The intensity of the phenomenon has appeared to him to correspond to the quantity of non-butyrateous globules. We shall return again, further on, to this observation.

" He has observed that the colostrum possessed an alkaline reaction upon the red paper of tournsol, as well as normal woman's milk, in which already M. Payen had recognized this property. According to M. Donné the colostrum passes gradually to the state of normal milk in the mammary organs.

" Although there are differences between the colostrum and the normal milk of the goat and ass, yet these are far from being so pronounced as the differences we have just stated between the colostrum and the normal milk of woman. And it is a notable fact, that the colostrum of the ass, which contained agglomerated globules and mucous globules, did not undergo any change in its liquidity, when ammonia was added to it.

" M. Donné has found in the milk of a nurse, whose right breast was engorged, agglomerated globules, mucous globules as in the colostrum; and he has ascertained that, as in this latter, it assumed a jelly form by the addition of ammonia.

" In fine he has recognized in woman's milk the existence of pus globules, which distinguish themselves from butyrateous globules, by their insolubility in ether, and their instantaneous solubility in the alkalies. The viscosity which this milk assumed by its mixture with these latter bodies, was not at all surprising, since Grasmeyer has given this property as one of the distinctive characteristics of pus.

" The figures of the globules of normal milk, of the agglomerated globules, of the muriform globules, of the mucous and of the purulent globules, which M. Donné has added to his pamphlet, compared with the globules observed by the microscope in specimens of milk, will be a means of recognizing whether these are in a normal state; and still more, according to him, thus if these samples are produced from a similar kind of animal, we shall be able to judge of their nutritive quality, by the proportion of the normal globules, by reason of that which chemical analysis seems to indicate, viz. that in the different specimens of milk from the same species of animal, the immediate principles, such as caseum, sugar of milk, are proportional to the butyrateous globules. Lastly many observations have induced M. Donné to think, that there is a relation between the health of the offspring and the globular matter which the microscope discovers in milk, so that the milk of a nurse, which presents in the second month after delivery, globules strange to the constitution of normal milk, compromises the health of the infant to which it is given."

Examination of the Note of M. Donné, on the Milk of Cows attacked by the Cocote.—This note not having been read or printed in the minutes of the sittings of the Academy, we shall present an extract of it.

Observations.—On the first day of the disorder, the milk does not differ from normal milk, when we examine it with the microscope; but on the following day we perceive in it agglomerated butyraceous globules; and the third day, M. Donné has recognized in it the four sorts of globules of the colostrum of woman, as well as the property of becoming viscid by ammonia. In short it has appeared to him that the milk, by changing itself, lost more and more of its alkalinity tested by red tournsol paper; at least this is what he has observed in the same cow's milk.

The milk of another cow has presented to him, at the end of seven days' disease, a strong proportion of agglomerated globules, of muriform globules, and of mucous globules. This milk, strongly alkaline became, by its mixture with ammonia, viscous and kneady. On the tenth day, one of the dugs was engorged, and seized with the affection which the dairies call "*cru*;" the milk that was drawn from it was greenish, excessively fetid; and we made the remark that the sulphurated odour which it exhaled participated of that of the acids of butter, and of that of acids developed by the putrefaction of azotized matters. M. Donné recognized in this milk the presence of purulent globules; but remarkable as a fact, the milk extracted from the other teats that the "*cru*" had not affected, was white and without bad odour; however it was altered, for it contained some lumps: the microscope therein detected, not only globules of colostrum, but also purulent globules; ammonia thickened it into a jelly; lastly it reddened tournsol paper.

Consequences.—M. Donné concludes from these observations:

1st. That by means of the microscope, it is easy to distinguish the milk of cows seized with "*cocote*" from normal milk, since the first contains globules very different from the isolated spherical butyraceous globules, which characterize the second.

2nd. That there is the greatest analogy between the globules of colostrum, and the globules of the milk of cows attacked with the "*cocote*."

3rd. That the milks extracted from the different teats of the same cow seized with "*cocote*" may be very different, if the glands which secrete the milk are in one of the dugs (*trayons**) under the influence of a local affection.

The microscopic examination of two specimens of milk, presented to the Academy by M. Donné in support of his note, repeated by many members of the Commission, and particularly by

* *Trayon* is used for the mammary gland, and the term *pis* for the true teat or nipple; they are not always strictly used by our authors, Rayet, Robiquet, Donné, and Chevreul. *Trayon* in the present sentence strictly means a subdivision of the gland having a teat proper.—TR.

M. Turpin, have convinced us that there were extreme differences between these specimens and normal milk ; and that besides they differed from it by the property of inspissating and even gelatinizing with ammonia, conformably to the observations of the author of the note.

Researches of the Commission and of other Persons relative to the nature of the Milk of Cows attacked with the "Cocote."—From these researches we shall extract the following facts.

M. Robiquet has examined several specimens of milk coming from different diseased cows : were destitute of fetid odour, alkaline to tournsol paper, and assumed viscosity with ammonia ; observations conformable to those which the Commission has reported higher up. But M. Robiquet has remarked further :

1st. That acetic acid which, as every body knows, coagulates normal milk and precipitates the caseum thereof, only produces in the morbid milk, a troubling hardly perceptible. This fact induces M. Robiquet to think that this milk had undergone an alteration, which might spring from the influence of the alkaline salts of the serum. 2nd. That the matter which gives to the morbid milk, property of becoming viscid by ammonia, is in suspension and not in solution ; for when this milk is well filtered it has lost this property, and if it retained in solution albumen like the specimens examined by M. Robiquet, which proceeded from the same cow, this albumen would coagulate by heat. This opinion is conformable to what M. Donné has advanced, relatively to the connexion between the property of inspissating with ammonia, and the abundance of the globules in the diseased milk, that is to say, the abundance of bodies which are not in solution.

M. Lassaigne has been enabled to multiply to a great extent his experiments, and observations on the sick cows of the herd at Alfort, in order to convince himself of the difficulty of characterizing, by means of chemical properties and reactions, the milk of these cows, for the reason that they may differ much one with another. He admits, however, as a character the viscosity that the morbid milk acquires by ammonia.

We give "*en resume*" what M. Lassaigne thinks of the influence that the "cocote" has exerted over the cows of the Alfort herd, relatively to the secretion and the nature of the milk : the secretion of the milk has been diminished, the milk was less buttery, more aqueous ; it has appeared more alkaline when the epizootic had most intensity, than on the decline of the malady ; it contained butyraceous matter, caseum, and sugar of milk, like normal milk.

Lastly, M. Lassaigne has examined a very remarkable milk coming from a cow seized with another disorder than the "cocote ;" this milk, very alkaline, issued troubled from the teat of the animal, and soon reduced itself into albuminous serum and into a solid matter, which appeared to M. Lassaigne formed of coagulated albumen and of fibrine.

To conclude, the characters we have given in order to distinguish normal milk from that of cows affected with cocote are as follows :

1st. Normal milk is perfectly liquid, and if it has not been recently drawn, it suffices to agitate it in order to make it assume an homogeneous aspect, since the butyraceous globules are then uniformly distributed in the serum.

2nd. Normal milk is perfectly mobile in its parts, as a fluid would be, deprived of viscosity.

3rd. When we inspect it in the microscope, it only presents spherical globules of a beautiful transparence, as is the watery liquid in the bosom of which these globules move with rapidity by the slightest impulse.

4th. Normal milk, far from inspissating by ammonia, would appear to take on more mobility and to lose its opacity.

5th. Exposed to the fire it does not coagulate as does albumen, and by boiling it does not exhale the decided sulphurous odour which this latter exhales.

6th. The odour of normal milk is very light.

7th. It is white, sometimes tinged bluish or yellowish.

8th. We shall speak further on of its action upon coloured reacting papers.

After what precedes, it is evident that when studying under the preceding relations a given natural milk, we shall arrive at the realizing of differences in the properties that we have just recognized, as characters of normal milk; we shall conclude from it that the first has undergone the influence of a cause foreign to the normal state.

1st. *Deficiency of homogeneity.* It cannot be sufficiently impressed, that certain milk presents, on its exit from the teat, an abundant solid matter, composed, according to M. Lassaigue, of albumen coagulated and of fibrine. But this milk is rare, and cannot be considered as an essential product of the "cocote," since the cow which has presented it during the epizootic, gave it on the 21st February, a date when for a month past there had been no cocote. However we do not say that cows affected with this malady may not present a flocculent or fibrinous solid matter, since three samples of milk originating from these cows have offered it to our examination.

2nd. *Deficiency of mobility or of fluidity.* It is very well perceived when we press a drop of milk between two plates of glass, in order to submit it afterwards to the microscope; it flows with difficulty, and does not extend itself in the manner of an homogeneous liquid.

3rd. *Presence of globules distinct from the globules of normal milk.* The agglutinated globules, the muriform globules, the mucous globules, and the globules of pus perfectly distinguish the milk wherein they are found from ordinary milk.

4th. *Inspissation by ammonia.* This character, witnessed by M. Donné, has been re-observed in all the milks arising from cows decidedly affected with cocote, examined by the Commission, M. Robiquet in particular, and by M. Lassaigue.

5th. *Coagulation by heat.* The coagulation by heat, indicated as a character of the milk of cows attacked with the cocote, is altogether fallacious, for we have not observed it in the milks which contained solid fibrinous matter, which wanted fluidity, which contained globules foreign to normal milk; in short, which inspissated considerably with ammonia. We have ascertained besides, that a morbid milk, not coagulable by heat, did not give this property to normal milk to which it was added.

6th. *Fetidity.* This character, easy to verify, is not essential to the milk of cows affected with cocote, since the greater number of specimens which we have examined were inodorous, or almost inodorous.

7th. *Colour.* A greenish yellow colour, a reddish colour, announce a milk altered or mingled with blood. A free yellow colour, may belong to a normal serum; in this case, it does not produce any coloured deposit in the milk, left to itself, as this takes place in that which owes its colour to blood globules.

8th. *Alkalinity.* This characteristic is decidedly bad for the distinction of morbid from normal milk, since we have alkalinity verified, according to M M. Gay Lussac, Darcet, Payen, and Donné, in several of milk of cows, goats, and sheep in the normal state, on the 21st of February in the School of Alfort.*

* In another part of the report, the learned reporter expresses himself thus: " Nothing is more suited to demonstrate how vague is our actual knowledge of normal milk, than the difficulty itself that we have experienced when it became a question to define, in this report, its action upon coloured reagents. Macquer (*Dict. de Chimie*) says that the normal milk of a frugivorous animal is neuter. Bouillon Lagrange (*Ann. de Chimie*, t. 50, p. 273) states that milk, recently drawn, reddens tournesol paper. M. Thenard (*Ann. de Chimie*, t. 59, p. 280) accords to it the same property on its exit out of the mammary glands; Thompson and Berzelius partake of his opinion. In a journey in 1826, that M M. Gay Lussac and Darcet made in Belgium, they recognized alkalinity on the issuing out of the milk from the teat, in forty cows.

M. Payen affirms the alkalinity of many specimens of woman's milk, and the neutrality of the milk of the goat. M. Lassaigne having examined the milk of a Swiss cow, twenty-two days before she was laid in the straw, found the milk, or the liquid which represented it, alkaline; but eleven days after this experiment the milk was acid, and this property persisted in the milk after delivery or calving. In fine, quite recently, M. Peligot and M. Lassaigne have considered acidity as a property of normal milk.

We have considered that this difference of opinions upon a fact easy to verify, was a powerful motive for adding some new observations to those we have just brought forward. In consequence, a member of the Commission was despatched to the Veterinary School at Alfort, where M. Lassaigne has had the politeness to put him in a position to ascertain the following facts:

The milk belonging to three English cows, of which one had calved eight and the others six months ago, directed on its exit from the teat upon tournesol paper, caused it to pass to blue. It was then alkaline. The milk of two goats which had been laid in the straw ten months before the experiment, was alkaline.

Lastly, two Merino sheep of pure blood, taken in a flock of forty dams, of which one had lambed two months, and the other three months before the experiment, yielded milk likewise alkaline.

Thus, we have seven specimens of milk taken from seven individuals belonging

9th. *Non precipitation of the milk in flocculi by acetic acid.* We mention this character, observed by M. Robiquet in many specimens of morbid milk. The Commission regrets not having subjected all the samples of milk which it has examined to this kind of test.

Definitively, if we seek the characters of milks proceeding from cows affected with the cocote properly so called, they have presented properties different from those of normal milk, we find them in a visible deficiency of homogeneity, in a deficiency of mobility or of liquidity, in inspissation by ammonia, that is to say, in three properties dependent upon one or many matters contained in the morbid milk, in a solid state, and finally in the presence of globules which are not met with in normal milk.

If we recall now, on the one part, that the flocculent or fibrinous matter that we have found in the milk of cows affected with cocote, was far from being there in as great a proportion as that of matters mentioned by M. Lassaigue, under the names of coagulated albumen and fibrine, in the milk of a cow seized with an affection very different from the cocote, and on the other hand that the globules of the milk of cows attacked by the cocote, are found in the colostrum and in different morbid milks, it is evident that we cannot characterize the cocote by a specific chemical constitution.—A. B. *Journal de Pharmacie.*

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY.

(CONTINUED.)

Twenty-first Meeting, April 27, 1839.

Mr. CARMICHAEL in the Chair.

1. *Gastritis; Hydrocele; Ossification of the Tunica Vaginalis.*—

Mr. Carmichael exhibited the recent parts in this case; the stomach was distended with air, and reached down to the umbilicus; the

to three different kinds of animals which are alkaline; does it follow that milk always possesses this property in the normal state? we should be tempted to draw this absolute conclusion from our experiments, and from those analogous ones made before, which we have cited; but if we consider that chemists, known for their exactitude, state that they have themselves verified the neutrality and the acidity of milk on its leaving the breast; that M. Lassaigue has upon the same animal, subjected to a constant regimen of alimentation, found at different epochs alkalinity at first, and then acidity of the milk, the Commission does not think it incumbent on it to decide the question: it will confine itself to remarking, “that the habitual state of normal milk appears to be a slight alkalinity, and that it remains for experimenters to determine if there are circumstances where the milk may become acid without its being possible to recognize in the animal which has produced it the first symptom of disease.”

It is understood that we only speak of milk at the moment of its escape from the mammæ of an animal judged to be in good health; for we know that a disease or a sudden affection has rendered the milk acid.

mucous membrane throughout its whole extent was soft and vascular, and covered with a thick tenacious mucus; the liver was enlarged and of the colour of a nutmeg. Mr. Carmichael also exhibited a remarkable specimen of hydrocele, taken from the same individual, the tunica vaginalis was completely converted into bone, and contained a dark-coloured fluid, thick and grumous; the hydrocele was about as large as a hen's egg, and lay altogether in front of the testis, which was sound in structure, and of natural size. The patient from whom the specimens were taken, was a man about fifty, remarkable for his obesity, and of rather intemperate habits; ten days before his death he was seized with severe vomiting of a bilious matter; he had also marked symptoms of hepatic disease, the vomiting became incessant, and all means failed to allay the irritability of the stomach. (*Museum, Richmond Hospital.*)

2. *Syphilis in the New-born Infant.*—Dr. Churchill presented a drawing of an infant born with syphilis; the skin was covered with a copper-coloured eruption; the mother of the child had contracted the disease from her husband; she had gonorrhœa and inflammation of the fauces; she stated, that she felt the motions of the child a few days before delivery; it was a breech presentation, but the labour was short and easy. Dr. Churchill had no opportunity of examining the internal organs of the child.

3. *Horny Excrescences removed from the Face.*—Mr. Carmichael exhibited two very remarkable specimens of horny tissue, which grew from the upper lip of an individual of about sixty years of age; they had been growing for six years, and had attained a very large size; they were successfully removed by Mr. Pierce, by whom they were presented to the Society.

4. *Bronchocele.*—Professor Harrison exhibited a fine specimen of this disease, taken from the body of a female of very nervous temperament, and who had suffered from painful menstruation for many years; whenever the menstrual flux was deficient there was a remarkable increase in the size of the bronchocele; but when the former was restored, the latter decreased. The tumour was as large as a melon, and presented a cellular structure, the cells being filled with matters of various density and consistence; some being firm and solid, others filled with a brown fluid; the vessels supplying the gland were very large and numerous. Professor Harrison also exhibited a polypus, occupying the os uteri, and taken from the same individual. The bronchocele made its first appearance when the patient was eight years old, and continued to enlarge till she was eighteen, when it ceased to grow. Professor Harrison mentioned, that each of the patient's sisters had bronchocele, while the male portion of the family were free from it. (*Museum, Trinity College.*)

5. *Hydrocele of the Neck.*—Mr. Smith presented two preparations illustrative of this disease; in one, the tumour, as large as an orange, lay above the thyroid gland in front of the larynx; it contained a clear serous fluid; the cyst was of considerable thickness; its interior smooth and polished. In the other case, the tumour oc-

cupied the whole of the front and left side of the neck; the fluid which it contained was turbid, and the lining membrane of the sac thick and flocculent. Mr. Smith detailed the history of this case, as published by Dr. O'Beirne, in the *Dublin Journal of Medical Science*, vol. vi., p. 10; the patient died of erysipelas, about two years after the appearance of Dr. O'Beirne's excellent essay upon *Hydrocele of the Neck*. (*Museum, Richmond Hospital*.)

6. *Pulsating Bronchocele*.—Mr. Smith also exhibited a drawing of a large bronchocele, which was preceded and accompanied by violent palpitation and general arterial excitement; there was an aneurismal thrill and bruit de soufflet in the tumour. A remarkable prominence of the eye-balls, generally present in this form of bronchocele, was admirably depicted in the drawing. Mr. Smith also drew the attention of the meeting to several specimens of hypertrophy of the thyroid gland, with dilated and tortuous arteries, and connected with hypertrophy of the heart. (*Museum, Richmond Hospital*.)

7. *Osteosarcoma of the Jaw*.—Mr. Cusack said he had an apology to make for bringing forward a specimen, which did not belong to that department of pathological anatomy, which the society was formed to illustrate; but he was anxious to exhibit it, as shewing the mode in which osteosarcoma commences. It was a morbid specimen, taken from an ox, and if examined carefully, the members would observe in it small deposits of a peculiar kind of structure, which Mr. Cusack regarded as the embryo of the osteosarcoma. These appeared to have been deposited at various points, eating away the bone by degrees, and converting the whole into a softened structure. A comparison with a human jaw-bone in which the same disease was present, shewed a very remarkable similarity. The deposition always commences in small points, and where the disease has existed for some time, so as to form a mass. The new deposits will be found separate and detached. In other situations the bone is wholly absorbed, and in its place there is a congeries of cells filled with soft matter, resembling that of true cancer. Each point at which the disease commences is a vascular point, around which the bony tissue is absorbed, leaving a cell in which the soft matter is deposited. The absorption of bone goes on in this way increasing, until at length the disease makes its way to the surface; the external layer of bone gives way, and the fungous mass becomes exposed to view.

8. *Cancer of the Penis*.—Mr. Cusack exhibited the recently removed parts in a case of this disease in which he had operated, and remarked on the difficulty which sometimes attends the diagnosis between cancerous and venereal diseases of the penis. He knew of cases in which the penis had been removed, the surgeon mistaking venereal warts or vegetations for cancer.

9. *Cirrhosis of the Liver, with Chronic Gastritis, and Perforation of the Stomach*.—Dr. J. Crampton exhibited the recent viscera in this case. The liver was greatly enlarged, and pressed on all the adjacent viscera, depressing the kidney and pushing the diaphragm upwards. The right lung was much reduced in volume, solidified, and presenting a few scattered tubercles in the upper portion. The anterior

portion of the stomach presented a remarkable softening of the mucous membrane, and a perforation existed, but no escape of the contents of the stomach had taken place, from the base being closed by the surface of the liver, to which it was adherent.

10. *Caries of the Hip Joint in the Infant.*—Mr. Ferrall said, he was about to exhibit a specimen of caries of the hip joint in an infant only four months old. In this case the disease was of six weeks' standing, and had commenced in the form of a large fluctuating tumour over the hip. The limb was kept flexed on the pelvis, and the child seemed to feel great torture whenever it was moved; it was also remarkable, that the tumour became greatly distended during a fit of crying. About three days after Mr. Ferrall had seen the child, the mother brought it to him again, and shewed another large fluctuating tumour on the back of the neck. He then lost sight of the child for some time, and on making inquiry, found that it had died. The account given by the mother was, that it had enjoyed good health until it had been vaccinated, when it began to decline, and about a fortnight afterwards the tumour of the hip joint appeared. On opening the joint, it was found to consist of a large sac filled with thin, purulent matter. This sac projected through the hole into the pelvis, a fact which accounted for the enlargement of the tumour, during a fit of crying. The head of the femur was completely destroyed; there was nothing left of the orbicular ligament, and the carious end of the femur rested on the edge of the acetabulum. There was no disease of the spine, nor could any tubercles be discovered in the lungs. The tumour of the neck was also filled with pus, which had made its way as far as the anterior mediastinum. The most remarkable points connected with the history of this case, were the occurrence of the hip joint disease at such an early period of life, and the absence of tubercles of the lung, or any of the other phenomena of the scrofulous diathesis. Another curious point was, the impulse communicated to the tumour of the hip, when the child cried, and which was afterwards explained by the projection of the sac into the pelvis. Mr. Ferrall said, that it might be necessary to state, that in this case the true pustule did not form on the arm after vaccination; but instead of it a large boil, which suppurated unhealthily. This would seem to countenance the supposition, that some infection of the blood had occurred, that the matter of the vaccine sore was absorbed, and that this was the origin of the purulent deposits in the hip joint and neck. On the other hand, there were grounds for suspecting, that the formation of matter in the hip joint was merely a coincidence, and that disease might have been going on there for some time before vaccination. Mr. Ferrall said, he was inclined to think, that the affection of the hip joint was an original disease, and that it had no connexion with the results of vaccination. Some time after the occurrence of this case, another child was brought to Mr. Ferrall, with caries of the spine, unaccompanied by any trace of scrofula in the system, nor could any sign of the scrofulous diathesis be discovered in either of the child's parents, except that the mother had also laboured under a similar affection of the spine. (*Museum, St. Vincent's Hospital.*)

Twenty-second Meeting, May 4, 1839.

Professor HARRISON in the Chair.

1. *Compound depressed Fracture of the Cranium, Hernia Cerebri.*—Mr. Lynch exhibited the recent parts, and a drawing of the appearances in this case; the right parietal bone was fractured and depressed; the fracture traversed the sagittal suture, and extended into the orbit; the dura mater was in a state of slough, and covered with purulent matter. The cerebral substance corresponding to where the bone had been depressed, was softened, and mixed with coagulated blood.

The patient, a female, aged 30, was admitted into Jervis-street Hospital, February 26th, 1839, with a large wound of the scalp, and in a state of insensibility. Ten days after the receipt of the injury, she was seized with paralysis of the left arm and leg; a week after which she was trephined. The operation was followed by paroxysms of shivering, constant pain in the head, and occasional delirium, and after the lapse of six days a fungus sprung up from the opening; it consisted of cerebral matter mixed with blood; twenty-one days after the operation an elastic, puffy tumour of the scalp appeared on the other side of the mesial line, two inches from the fungus. The shiverings and delirium continued, and the pulse became feeble and rapid. She died on the 30th of April, sixty-three days from the date of her admission, and forty-six after the operation.

2. *Diffused Aneurism of the Subclavian Artery.*—Mr. Cusack exhibited a specimen of this disease: the lesser pectoral muscle was expanded over a large mass of coagulated blood, which filled the axilla, extended along the clavicle towards the sternum, and diffused itself among the muscles and cellular tissue of the arm and side; the artery was pushed forwards by it so as to render it impossible to pass a needle round it without entering the sac. The sac had burst into the axilla; the pericardium was found to adhere to the base of the heart, and the arteria innominata and left carotid presented numerous small aneurismal dilatations.

Seven weeks previous to death, the patient first complained of pain in the arm, and became sensible of the existence of a pulsating tumour in the axilla. When Mr. Cusack saw him, the pulse at the wrist was almost imperceptible; the aneurismal sac had given way, and he was suffering the most intense pain.

The usual operation for securing the subclavian artery in its third stage was attempted, but upon endeavouring to pass the needle round the vessel, the aneurismal tumour was penetrated; an alarming gush of blood took place, which was arrested by plugging the wound with sponge. The man survived the operation for ten days; his death was then occasioned by a fresh effusion of blood into the axilla.

3. *Diffuse Inflammation occurring after Delivery.*—Dr. Evory Kennedy exhibited the recent parts in this case. The interior of the uterus was highly vascular and covered with purulent matter, particularly about the cervix, and where the placenta had been at-

tached on the external surface, purulent deposits were seen in the uterine veins. The superficial veins of the thighs were filled with lymph, and small abscesses existed in the cellular tissue. The subject of the case was a woman, aged 32, who had been admitted into the Lying-in Hospital about ten days previous to her death; nothing remarkable occurred until the fourth day after delivery, when a patch of diffused inflammation was discovered on the posterior surface of the thigh. The patient stated that she had been labouring under this affection for a few days before admission. She died on the seventh day after delivery.

4. *Injury of the Head occurring during Labour.*—Dr. Kennedy also exhibited a preparation, shewing a remarkable depression of the parietal bones, produced during labour by a contracted state of the antero-posterior diameter of the pelvis. (*Museum, Lying-in Hospital.*)

5. *Atrophy of the Uterus.*—Dr. Kennedy also exhibited a remarkable specimen of the atrophy of the muscular structure of the uterus, the organ of which was quite disaphanous; it likewise contained a polypus of considerable size: the preparation was taken from the body of a woman aged 45, who had borne several children.

6. *Caries of the Bones of the Pelvis.*—Mr. Adams brought forward a specimen of caries of the ilium, accompanied with extensive abscesses in the neighbouring parts. Having made a few observations upon the treatment, Mr. Adams next drew attention to a remarkable conformation of the liver, found in the same subject, a female æt. 64. This organ was deeply indented by the margin of the ribs, the chest was narrow below and broad above, and the apex of the lungs was elevated above the clavicle; these alterations were owing to tight lacing. Mr. Adams also shewed a specimen of atrophy of the eyeballs extending to the nerve of the same side, and optic tract of the opposite side. (*Museum, Richmond Hospital.*)

7. *Fungus of the Bladder.*—Mr. Ferrall exhibited a preparation of a large fungus which filled the cavity of the bladder, and projected into the vagina through a large opening in the inferior fundus of the organ: the fungus had also completely blocked up the opening of the ureters; these tubes were dilated to five or six times their natural size, and the kidneys were pale and atrophied. The subject of the case was a woman, æt. 40, who had been admitted into St. Vincent's Hospital, for a supposed attack of uterine hæmorrhage; she suffered from pains in the loins and pelvis, had frequent desire to pass water, and the urine was turbid and tinged with blood: the pain in passing water was increased while expelling the last drops, and some time before death she was unable to void urine at all. The chief interest connected with the case was its resemblance in some symptoms to a case of calculus, and in others to uterine disease, and the explanation of both groups of symptoms afforded by the presence of the fungus in the bladder. (*Museum, St. Vincent's Hospital.*)

8. *Inflammation of the Uterus.*—Dr. Churchill exhibited a specimen of inflammation of the uterus, particularly marked towards the cervix of the organ; there were likewise distinct traces of phle-

bitis in the uterine veins; the disease was produced by the tedious and difficult passage of the child's head; the labour was attended with great suffering, and followed by alarming hæmorrhage.

Twenty-third Meeting, May 11, 1839.

Mr. FERRALL in the Chair.

1. *Ulceration of the Throat extending to the Lingual Artery; Death by Hæmorrhage.*—Dr. Duncan presented the recent parts in this case. The patient, a young man, had been under treatment at the Adelaide Hospital, for ulcerated sore throat, for some time, when he was suddenly attacked with hæmorrhage from the throat, which returned twice in the course of a fortnight. He had left the hospital, but was readmitted, and on the following day the bleeding returned with greater violence, and he was much exhausted. The ulceration was found to have attacked the right lingual artery, which presented a perforation capable of admitting a large sized probe. The os hyoides was found to be carious. Dr. Duncan alluded to cases of the same kind which occurred under the care of the late Dr. McDowell, in one of which the external carotid had been tied with perfect success.

2. *Softening of the Anterior Column of the Spinal Cord in its Cervical Portion.*—Dr. Power begged to draw the attention of the Society to a well marked and recent specimen of acute softening of the anterior column of the spinal cord. The patient, a woman, aged 50, was suddenly attacked with paralysis of motion in the upper and lower extremities. The bladder and rectum were unaffected; a slight power of motion remained in the limbs. There was no loss of sensation; no fever, headach, or disturbance of intellect. *Sensation in the paralyzed portions was perfect.* Soon afterwards she was attacked with dyspnœa, and her breathing became diaphragmatic: ultimately the diaphragm became paralyzed, and death took place with great dyspnœa. The spinal column was opened on the following day, and the cervical portion of the medulla spinalis was found softened to a great degree. (*Museum, Richmond Hospital.*)

3. *Secondary Fistulæ of the Pleura in Cases of Empyema and Pneumo-Thorax.*—Dr. Stokes exhibited a drawing illustrative of this pathological condition, which had, he believed, been first described by Dr. Houghton in his article on Pneumo-thorax, in the Cyclopædia of Practical Medicine. In the case then under consideration, the pleura presented the following appearances. The original fistula from within outwards, communicating with the cavity in the lung was evident. But in addition to this opening, were several others, which had formed from without inwards; these presented oval patches, shewing perforation of the serous membrane and subjacent structures, at the base of which were several circular openings, communicating with minute bronchial tubes, but not with any distinct tuberculous abscess. This condition seemed to require a very considerable duration of disease for its production; and Dr. Stokes was of opinion that these secondary fistulæ would account for some of

the peculiarities in the physical signs of chronic empyema and pneumo-thorax. (*Museum, Park-street.*)

4. *Fungous Disease of the Bladder, with Cancerous Ulceration of the Vagina.*—Mr. Ferrall laid on the table the preparations in this case. The patient had been admitted into St. Vincent's Hospital, labouring under the effects of repeated hæmorrhages from the vagina, which had produced all the usual phenomena of anemia. On examination Mr. Ferrall found that the ulceration was confined to the vagina; she frequently complained of pains in the region of the bladder, with frequent desire to pass urine. She died from exhaustion, and on dissection it was found that the vagina was the seat of external cancerous ulceration. The uterus was healthy, with the exception of the os tincæ, to which the ulceration had extended; continuous, however, with the cancerous mass, was a large fungoid or medullary tumour, which penetrated the roof of the vagina, and entered the bladder, forming in the cavity a large fungous mass.

Mr. Ferrall exhibited drawings of the case, and remarked that the disease simulated the corroding ulcer of the uterus and vagina, which is said to be distinguishable from cancer, from its being accompanied with a moveable state of the uterus. In this case, however, the uterus was quite moveable, although the ulceration was decidedly cancerous. With the view of shewing the difficulty of distinguishing fungus of the bladder under some circumstances, Mr. Ferrall begged leave to exhibit preparations and drawings of another case. A young woman was admitted into hospital with symptoms of chronic inflammation, or as it has been termed, catarrh of the bladder. After some time the urine became purulent, and on examination it was found that there was a tumour in the hypogastrium which gradually advanced towards the surface, with a tuberculated feel, some parts being hard and unyielding, others soft and elastic. The occurrence of this tumour, the presence of albumen in the urine, and the frequent desire to pass water, induced several persons to look upon it as fungoid disease of the bladder. Two circumstances, however, seemed to throw a doubt on this diagnosis. In the first place the urine was not permanently albuminous; sometimes the albumen was absent for three or four days together, sometimes it was present, and with it there was generally an admixture of pus. In the next place there was a feeling of fluctuation in the tumour, so evident, that Mr. Ferrall could not be persuaded but that there was matter underneath. An incision was made over it, and about twenty ounces of foetid pus were discharged. The patient subsequently fell into a state of great exhaustion, and died four months afterwards with symptoms of phthisis. On examination after death, a large abscess was found in the pelvis, extending in various directions, but not communicating with any of the viscera, except the bladder, into which it had formed a small opening. The intermitting occurrence of albuminous urine was explained by the fact, that from the small size and peculiar form of the opening, it was very liable to become occasionally obstructed, so as to interrupt the passage of the contents of the abscess into the bladder, and in this way to render the occurrence of albuminous urine an intermittent phenomenon. The

case was interesting, as shewing how an abscess in the pelvic region communicating with the bladder, may simulate fungoid disease of that organ. (*Museum, St. Vincent's Hospital.*)

5. *Medullary Tumour of the Pancreas, communicating with the Stomach; Death by repeated Hæmorrhages into the Digestive Tube.*—Dr. Corrigan said, the preparation he was about to exhibit was interesting as a contribution to abdominal pathology. He would first give a short history of the case, and then shew the preparation. A gentleman, who had been previously in good health, with the exception of some slight feeling of uneasiness in the abdomen, while walking along the streets, about a month since, felt suddenly weak and faint, with an inclination to discharge his bowels. He was brought home in a car, and on arriving there, passed a quantity of pure blood from his bowels. He recovered from this, and returned to his usual avocations; but after some time was again attacked with a sensation of faintness and a discharge of blood per anum. These discharges became gradually more frequent, and occurred at shorter intervals. About five days before his death he was visited by Dr. Corrigan, who found him quite pale and blanched, with a quick and weak pulse. But he had what Doctor Corrigan looked upon as a very unfavourable sign, namely, a persistent bruit de soufflet in the heart and larger arteries. The discharge from the bowels, when seen by Dr. Corrigan, was not the same as it had been at first, but rather resembled the evacuations seen in cases of melæna. Dr. Corrigan saw him for the last time, about three o'clock, P. M., on the day of his death. During his visit the patient observed, that he thought he felt the blood coming. He was advised to be extremely cautious, and keep himself perfectly quiet. He disobeyed this injunction, and attempted to sit up during the course of the afternoon. He had not been many minutes in the erect posture, when he leant back on his pillow and expired. Half an hour before his death he took some beef tea, with relish, and had taken it several times during the course of the day; in fact, the appetite was quite good, and there was no interruption whatever of the digestive function, nor was there any symptom of serious or organic disease of the stomach. Dr. Corrigan said that it would be necessary to state, that on one or two occasions, when this gentleman felt the weakness coming on he had some sickness of stomach, and threw up a few mouthfuls of blood. His case was regarded by some as one of melæna; but there was this difference, he had not suffered any previous illness, or derangement of the digestive system; and the evacuations, though bloody, and even dark, were not fetid. Again, neither the discharges, nor the calls to them, were preceded or accompanied by those colicky pains, so frequently observed in cases of melæna. Finally, the discharges came on each time with a sense of faintness, as if from internal hæmorrhage, and the blood, which was then passed, was pure, and unlike that observed in melæna. These circumstances appeared to be sufficient to distinguish the case. On opening the abdomen the intestines were found to contain a large quantity of blood, and the mucous membrane was deeply tinged with the colouring matter. The stomach appeared enormously distended, and filled the whole epigastrium. On slitting

it up, its cavity was found to be filled with a mould of the organ, consisting of a large mass of coagulated blood, which weighed more than two pounds. The interior of the stomach was healthy, except in the vicinity of the cardiac orifice, where a mass, apparently composed of coagulated fibrine, and distinct from the clot, already described, projected into its cavity. On tracing this further, it was found that a portion of the mucous membrane in this situation had been removed by the process of absorption, and that this projection corresponded with the surface of a large medullary tumour of the pancreas. From the surface of this, blood appeared to have been thrown out into a kind of cyst, formed by adhesions between the anterior surface of the pancreas and the posterior wall of the stomach, and became accumulated so as to press on the stomach, and produce absorption of its coats. In this way it was that the blood made its way into the stomach during each attack of hæmorrhage. The case was interesting, as affording a very remarkable specimen of a rare disease, and from the resemblance it bore to melæna. It was also a very singular fact, that the digestive function did not appear in any way impaired, and that the desire for food had continued up to the period of death. (*Museum, Digges-street.*)

6. *Caries of the Cervical Vertebrae*.—Mr. Smith begged leave to present two specimens of spinal disease, occurring under peculiar circumstances. The first case was that of a young man, remarkable for his bodily vigour. He was a fine healthy-looking soldier, of temperate habits. Some time ago he was attacked with fever, having been previously in perfect health; the fever was severe and protracted, and during its latter stages the cerebral symptoms were very prominent. After some weeks he became convalescent, but regained his strength very slowly, and began to perceive that he was gradually losing the power of his upper extremities, and that on two or three occasions he had some difficulty in passing water. In the course of about three weeks he became perfectly paralysed in both arms, and had paralysis of the bladder. It was remarkable, that from the commencement of his convalescence he had never complained of any symptom of spinal disease; his case went on in the usual way, and he died about a month after the first appearance of the paralysis in the arms. Mr. Smith made an examination of the body, assisted by Mr. O'Brien of the 7th Fusiliers. On opening the vertebral canal extensive marks of disease were found in the cervical region; the third and fourth cervical vertebrae were almost wholly destroyed, and the matter had made its way into the spinal canal, extending upwards as far as the base of the skull. It had also escaped in front of the spinal column, and had got into the posterior mediastinum. The pia mater was extremely vascular. On opening the chest both lungs were found to be filled with tubercles. The case was an extremely curious one in many respects. Previous to his illness this young man had been one of the healthiest men in the regiment, and never had any symptom whatever of pulmonary disease; in fact, with the exception of slight cough and dyspnoea, which were first observed three weeks before the fatal termination, he had no symptom of it up to the period of his death; and yet on dissection the lungs were found to be filled with

tubercles. He had no paralysis of the lower extremities. (*Museum, Richmond Hospital.*)

7. *Caries of the Dorsal Vertebrae*.—The second preparation exhibited by Mr. Smith, was also very remarkable; he had, in fact, never seen any thing like it. It was the case of a man, aged 21, who attributed his disease to an injury of the back, received while lifting a heavy sack of corn. He was admitted into hospital about eight months afterwards, for an attack of pleuritis. At the time of his admission he had some degree of tenderness and pain in the spine, and there was a projection of the vertebral column opposite to the seventh and eight dorsal vertebrae. He was treated successfully for the pleuritis, and left the hospital quite recovered. Eight months afterwards he was again admitted with symptoms of spinal disease. He was now labouring under confirmed hectic with cough, foetid breath, and copious purulent expectoration. He had a large tumour on the back lying between the base of the scapula and the spinous processes of the vertebrae, which had appeared suddenly ten days before admission. He stated that its extension was very rapid, and that it had attained its full size in two hours after its first appearance. Two days afterwards he had profuse purulent expectoration, and he observed, that immediately after this, the tumour of the back subsided in a very remarkable manner. Shortly afterwards an incision was made into the dorsal tumour, which gave exit to a large quantity of foetid pus mixed with air. It was also found that during the act of inspiration the tumour became enlarged, and bubbles of air escaped from its orifice; air also insinuated itself into the cellular tissue of the back, so as to render the parts about the abscess quite emphysematous. The patient then got paralysis of the bladder, and loss of motion and sensation in the lower extremities. He was then attacked with profuse diarrhoea and sloughing of the integuments over the sacrum, under which he sank rapidly. On examination after death, the bodies of the seventh and eighth dorsal vertebrae were found to be carious, the ulceration extending through the bone so as to lay open the spinal canal. An abscess was formed round the carious bones which communicated posteriorly with the abscess on the back, and anteriorly by a very circuitous route, with an abscess of the lungs which opened into the bronchial tubes. (*Museum, Richmond Hospital.*)

8. Dr. E. Kennedy wished to exhibit a very curious drawing, and regretted he could not shew the preparation from which it had been taken. The disease was one which might come under the denomination of phlebitic gangrene, and was almost unique, at least Dr. Kennedy had never seen any thing resembling it. It was characterized by extreme intensity and rapidity, death having occurred in about seventeen or eighteen hours from the commencement of the disease. The patient was attacked with symptoms of febrile excitement on the fifth day after confinement, and on the following day complained of acute pain in the calf of the leg, accompanied by some slight discoloration of the skin. The disease went on with extraordinary rapidity, the pain and discoloration ran up the limb, the pulse rose to 140, being at the same time weak and thready, and evident marks of prostration

began to appear. The disease extended up the limb, accompanied by discoloration and excruciating pain of the thigh; and in the course of seventeen or eighteen hours from the commencement of the disease the woman was dead. On examination the veins of the limb were found to be extensively inflamed and coated with lymph; and the muscles and cellular membrane of the calf in a state of disorganization, scarcely a trace remaining of the original appearance of the gastrocnemius and solæus. The uterine and iliac veins were also inflamed. Dr. Kennedy observed that the same state of parts was present in a case to which he had referred at a former meeting, and which had been illustrated by exhibiting the uterus of the patient. He thought that these cases were entitled to the denomination of phlebitic gangrene. (*Museum, Lying-in Hospital.*)

Twenty-fourth Meeting, May 18, 1839.

Dr. JOHN CRAMPTON in the Chair.

1. *Aneurism of the Abdominal Aorta; Infiltration of the cellular Membrane surrounding the Kidney with Blood.*—Dr. Green laid on the table the recently removed parts in this case. The patient had been for three years subject to attacks of pain in the epigastric region, which were occasionally so violent that he had to throw himself on his belly, and press his stomach strongly against the floor, for two or three hours at a time, in order to obtain relief; he never had spinal pains, cramps, or coldness of the feet; on admission into hospital a pulsating tumour was discovered in the epigastric region and extending to the left side. The pulsations as compared with those of the heart were very violent, the pulsations of the tumour were not accompanied by any bruit de soufflet.

The aneurism was of a moderate size, and presented an opening close to the coeliac axis, and it was remarkable that after death the tumefaction of the epigastrium had completely subsided, a fact which could not be explained by the amount of the effused blood, which was very small. Dr. Green considered that this indicated the existence of a contractile power in the sac itself; the sac had contracted adhesions with the ascending colon, the left kidney, and spleen; it was filled with concentric layers of fibrine, and was evidently of long standing. (*Museum, Richmond Hospital.*)

2. *Tuberculous Tumours of the Uterus.*—Dr. E. Kennedy laid on the table a preparation, with a drawing and cast, illustrative of this form of disease which has as yet not been properly distinguished. The patient, advanced in life, presented a tumour projecting beyond the orifice of the vagina, bearing some resemblance to polypus; but the fact of the mucous membrane of the vagina being discoverable over its whole surface, except in one spot, led Dr. Kennedy to doubt this opinion. It was then a question whether it might not be an inverted uterus; and a small orifice on the surface looked like the opening of a fallopian tube. The finger, however, could be passed up beyond the tumour. The patient, who had been labouring under peritonitis, on admission, died soon afterwards. The uterus was found displaced, and the round and broad

ligaments stretched considerably. The uterus was found to contain a number of tubercular growths extending upwards towards the brim of the pelvis, and downwards into the vagina, distending the mucous membrane of the uterus, and resembling at first sight polypous growths. One of the tumours sprung from the outer surface of the uterus, the others grew from the anterior of the organ. Dr. Kennedy observed that many cases which had been described as polypus of the uterus, were really examples of this disease. (*Museum, Lying-in Hospital.*)

3. *Great Enlargement of the Placenta.*—Dr. Kennedy next exhibited a placenta of extraordinary size. It was a single placenta; and the patient had not had twins. She had a contracted pelvis, and had been delivered by the crotchet at her first confinement; in the second she was delivered without instruments, but the edge of the placenta presented. The placenta occupied more than half the surface of the uterus, and was as large as that in a case of triplets which had been recently in hospital. (*Museum, Lying-in Hospital.*)

4. *Hypertrophy of the Liver in the Fœtus.—Hernia of the Lobe of Spigel at the Umbilicus.*—Dr. Kennedy also presented the recent parts in this case. The liver was of vast size, and exhibited tubercular excrescences. The membranes of the cord formed the sac of the hernia. (*Museum, Lying-in Hospital.*)

5. *Pleuritis terminating in Empyema.—Operation.*—Dr. Corrigan laid on the table a cast and preparations of the parts in *two cases* of empyema.

The patient had been admitted in January for an attack of acute pleuritis and placed under the care of his colleague, Dr. Hunt. The urgent symptoms were relieved, but his convalescence was slow and imperfect, and he left the hospital with symptoms of matter collecting in the left side of the chest, namely, absence of resonance and of the respiratory murmur, and dulness on percussion. In the month of March the symptoms of empyema became more decided; the heart was pulsating at the right side of the sternum, and there was moderate clearness on percussion over the upper part of the left side as far as the fourth rib, but below this the chest was perfectly dull. Combined with this dulness there was another symptom worthy of notice. While the side from the fourth rib downwards was quite dull on percussion from the point of union between the cartilaginous and bony portions of the ribs, between the latter and the sternum it was quite clear on percussion, and became absolutely tympanitic as percussion was made lower down. Towards the convexity of the ribs, the chest again became clear on percussion; so that passing from right to left there was first extreme clearness of sound from the sternum to a line drawn a little beyond the point of union of the bony and cartilaginous portions of the ribs, then dulness as far as the convexity of the ribs, and then clearness again. Another point which had been ascertained during life was, that the fluid was fixed in its situation, and could not move about freely in the chest. On making percussion in the erect posture, and then repeating the same operation after

the patient's hips had been elevated and his head and shoulders depressed, so as to reverse the perpendicular bearing of the chest, it was found that the line of dulness did not vary. On examination after death the lung was found to be compressed, but still in some degree permeable. Above, it had contracted adhesions to the costal pleura; these extended also along the convexity of the lung, which was attached to the ribs and diaphragm in the situation where the clear sound was perceptible below the axillary region. The occurrence of the tympanitic sound, over that portion extending in a line from the mamma to the sternum, was also explained. This phenomenon had been produced by the presence of the stomach in that situation, and a tympanitic state of that organ. Dr. Corrigan had taken a cast of the chest and abdomen with the parts *in situ*, from which it appeared that the larger curvature of the stomach had pressed the diaphragm upwards, and encroached so considerably on the cavity of the chest, as to be on a level with the left mamma. This tympanitic state of the stomach occurs occasionally in chronic cases, and tends to throw some degree of doubt on the results of an exploration of the chest; but in this case there was another cause in operation. The heart, which lay in the right side of the chest, during the latter stages of the disease, after the operation, moved a little towards its natural situation, and approached the left side as the matter was discharged; but from the extreme firmness and density of the sac, as well as the unyielding nature of the mediastinum, it could not regain its normal position in the chest. The consequence of this was, that as the lung was not able to move down to fill up the vacant space, the abdominal organs were pushed upwards, and the stomach ascended as high as the left mamma, giving rise to the remarkable clearness of sound already described. There was another point in the case deserving of notice. The man had all the constitutional symptoms of empyema; and in addition to these, symptoms of phthisis were discovered. He had emaciation, night sweats, hectic, with dulness on percussion over the right side, and gargouillement. The man in fact had all the signs and symptoms of phthisis with the exception of cavernous respiration. The case would at first sight appear to be one extremely unfavourable for operation; but the absence of cavernous respiration determined Dr. Corrigan to have recourse to paracentesis; and he was justified in this opinion by the results of the *post mortem* examination. There was no trace of tubercular deposition in the lung, and the purulent liquor which filled the bronchial tubes was merely a secretion from the mucous membrane. Another point to which Dr. Corrigan wished to refer, was the size of the opening. This was purposely made small to allow the matter to trickle out slowly. In this way the chance of irritation from the entrance of air was obviated, as well as the occurrence of another kind of accident, which had happened in a case witnessed by Dr. Corrigan, and from which he wished to warn all future operators. The preparation before him, which represented the lung in a carnified state, and surrounded by a mass of coagu-

lated lymph, was taken from the body of a man who had died shortly after the operation had been performed; in this case a large opening had been made, and the matter evacuated at once. Two or three hours afterwards, the patient became anxious, tossed about restlessly in bed, and said his chest was filling again. He then became quite faint, exhibited all the symptoms of a person sinking from hæmorrhage, and died in the course of a few minutes. On opening the chest the affected side was found to be quite free from purulent matter, but the whole cavity of the pleura was filled with blood, partly coagulated, partly fluid. When all the matter had been drawn off, and the opening closed, the case might have gone on very well if the lung had been able to expand and fill the cavity; but being incapable of doing this, in consequence of its carnified state, and the adhesions it had contracted, a vacuum was formed in the chest which acted on the lung in the manner of a cupping-glass, and caused the vessels of the lung and pleura to pour out blood until the man died of hæmorrhage. It might be supposed that some vessel of importance had been wounded in performing the operation, but this was not the fact, the hæmorrhage had been produced by the cavity of the sac acting on the lung like a cupping-glass. In the case before the Society, Dr. Corrigan had caused a small opening to be made; this allowed the matter to trickle away slowly, and obviated any risk of hæmorrhage. (*Museum, Digges-street.*)

6. *Aneurism of the Aorta with Bruit de Soufflet; Aneurism of the Aorta without Bruit de Soufflet.*—Dr. Corrigan said that he exhibited these specimens together, because they agreed in their pathological characters with one single but most important exception. Both were examples of aneurisms of the ascending aorta; one of them had been taken from the body of a woman named Hamilton, the other from the body of a man named Dunn. They agreed in their size, situation, pathology, and even in their diagnostic signs, with this exception, that in the case of Dunn the *bruit de soufflet* was never absent, while in that of Hamilton it was never present. In the case of Dunn the aneurism involved the mouth of the aorta; in the case of Hamilton it did not, there being a portion of the vessel, about an inch and a half from its mouth, perfectly free from disease. In Dunn's case too the heart was very large; in Hamilton's it was below the natural size. In Hamilton's case the valves and the commencement of the aorta being sound, and the action of the heart weak, there was no vibratory motion communicated to the parietes to give rise to bruit de soufflet; but in Dunn's case there was a flaccid state of the heart, with disease of the aortic valves. Dr. Corrigan thought that, as a general rule, bruit de soufflet would be found in all cases where the aortic valves were diseased, and that it would be absent where they were sound. (*Museum, Digges-street.*)

7. *Inflammation of the Knee Joint.*—Mr. Adams presented a specimen illustrating the effects of inflammatory action on the structures of the knee joint. The subject was a young man from the country, aged 19, who had neglected the disease until it was beyond

the reach of surgical skill. He had been a patient at the Richmond Hospital for the last three months. Some time after his admission it became evident that suppuration had taken place to a very great extent, and Mr. Adams proposed amputation, but he would not submit: but was anxious to have the abscesses opened; and this was done with temporary relief: but about six days afterwards violent inflammation came on, and assumed so threatening a character that the man was obliged to submit to amputation. On opening the joint the ravages of disease were very striking—pieces of loose cartilage, shreds of lymph, and false membrane, fistulous openings communicating with abscesses on the outside of the joint, and extensive stripping of the bones, which were covered with a kind of pultaceous lymph, were observed. Mr. Adams said that it was one of those cases in which, according to Sir B. Brodie, the disease originated in the cartilage; and there was at first scarcely any pain, but when the cartilage begins to ulcerate, the patient's agony is very great even on the slightest motion. The results of the case also tended to increase his fear of opening abscesses in the vicinity of diseased joints.

8. *Atrophy of the Neck of the Thigh Bone.*—Mr. Adams said, that much as the subject had been investigated, he thought the structure of the neck of the thigh bone had escaped observation. Very few seem to understand it properly; and no one has made a vertical section of the bone for the purpose of showing the importance of the bony arch between the head and shaft, and the strength which it confers on the bone in this situation. Mr. Adams had made this section in the young and healthy bone, and also in the old and altered bone, with the view of showing the great difference in the structure and appearance of the bony arch. Mr. Adams thought that it was the absorption of this arch which gave rise to the great liability to fracture of the neck of the thigh bone from trifling causes in persons advanced in life. He was aware that this had been denied, but whoever would examine preparations of impacted fracture, would find this structure greatly diminished. In these cases the bony arch is greatly reduced in thickness and strength. Mr. Adams exhibited specimens in illustration of this. A few days previously, a woman died in the House of Industry who had been bed-ridden for fifteen years; and Mr. Adams was anxious to examine the joint, thinking it would be a good opportunity of ascertaining whether this bony arch became atrophied from want of use. A section of the bone was exhibited to the meeting, which showed very considerable absorption of the arch. The interior of the bone was filled with yellow matter, and the compact tissue of the arch nearly gone. Another point connected with this case was, that in this woman the neck of the thigh bone, instead of being horizontal as in other cases, was raised upwards, and was not so horizontal as it would have been in a person who had used the limb in walking. Persons of high standing in the Profession had asserted, that the absorption of the arch had nothing to do with

fracture of the neck of the thigh bone; and it was this that had led Mr. Adams to make these observations. (*Museum, Richmond Hospital.*)

Twenty-fifth Meeting, May 25, 1839.

Professor GRAVES in the Chair.

1. *Dislocation of the Femur upon the Pubis.*—Mr. Smith exhibited a cast of a luxation of the head of the femur forwards and inwards upon the horizontal ramus of the pubis; the head of the bone formed a distinct prominence close to Poupert's ligament; the cervix rested upon the brim of the pubis; the femoral artery ran tortuously in front of the head of the bone, and the veins of the leg were varicose; the muscles of the glutæal region were atrophied. The injury had been inflicted sixteen years ago; its nature was recognized at the time, but the efforts to reduce the luxation proved fruitless. The limb was one inch shorter than the other; the individual could neither flex nor abduct it, and extension was very limited. The joint was now occasionally painful, and affected by changes in the weather. (*Museum, Richmond Hospital.*)

2. *Chronic Disease of the Bladder.*—Mr. Ferrall exhibited a drawing of the recent appearances in this case. The bladder was thickened in a remarkable manner; all its tissues were hypertrophied, and its cavity much smaller than natural; the uterus was also thickened, but without any appearance of cancerous deposit; the os tincæ was studded with a number of vesicular eminences, similar to those described by Dr. Montgomery; the walls of the rectum were similarly affected. The patient, a female, æt. 60, had been admitted into St. Vincent's Hospital, labouring under disease of the heart and anasarca. She had also constant desire to void urine, tenesmus, difficulty in passing fæces, and laboured under symptoms of uterine irritation. (*Museum, St. Vincent's Hospital.*)

3. *Tubercular Ulceration of the Bladder.*—Mr. Ferrall also laid before the Society a remarkable specimen of ulceration of the bladder. The ulceration had attacked almost the whole of the mucous membrane of the organ, and extended for some distance into the urethra. There was a large abscess between the rectum and bladder, but it had not formed a communication with either of these cavities. The kidneys presented the usual marks of tubercular disease; and the cortical substance appeared to be altogether destroyed. The patient was a boy aged fourteen, who was admitted at St. Vincent's Hospital, labouring under an extreme degree of urinary irritation, so as to be obliged to pass water every half hour. He had also the usual symptoms of phthisis, as cough, night sweats, emaciation, and purulent expectoration. In addition to pain in the bladder, sometimes of a very acute character, and constant desire to pass water, the prepuce was elongated; he passed occasionally a quantity of blood, and stated, that sometimes the stream of urine became suddenly stopped, and that the expulsion of the last drops was accompanied by an increase of pain. His symptoms, in fact, were so like those of

stone in the bladder, that Mr. Ferrall sounded him, and had him subsequently sounded by Mr. Wilmot, but without discovering any foreign body in the bladder. Mr. Ferrall being anxious to test the accuracy of the lad's statements, questioned him very closely, and discovered that he had made false representations as to the nature of his symptoms. He found, that although he suffered extreme pain in passing water, the pains ceased when the last drops were expelled. With respect to the other symptom, namely, that the stream of water would sometimes stop suddenly, he found that it was a mere voluntary effort, and not the effect of spasm or obstruction. The urine, which was at first acid, became afterwards alkaline and purulent, and then began to exhibit distinct traces of albumen. Incontinence of urine now came on, and it was necessary to place a vessel in the patient's bed, to receive the water, which flowed off at all hours of the day and night. A short time before death the patient was attacked with profuse diarrhœa, which lasted about a week; during its prevalence a remarkable change took place in the symptoms connected with the bladder. While the purging continued the symptoms of urinary irritation disappeared in a great measure; the pain and desire to pass water, as well as the purulent and bloody discharges, were greatly diminished; but when the diarrhœa was checked, all the symptoms connected with the bladder returned with increased intensity.

Mr. Ferrall observed, that the points most deserving of attention, as connected with the case, were the strong resemblance which the symptoms bore in the first instance to stone in the bladder, then the concurrence of albumen with pus in the urine; and lastly, the alternation of the symptoms of urinary disease with diarrhœa. The extension of the tubercular ulceration into the urethra was also remarkable; and Mr. Ferrall observed, that he did not know any case of the kind on record. (*Museum, St. Vincent's Hospital.*)

4. *Malformation of the Heart.*—Dr. Law exhibited the heart of a boy æt. four, shewing an open foramen ovale, the only openings into the left auricle were that of the foramen ovale and the openings of the pulmonary veins; it did not communicate with the left ventricle; the ventricular septum was deficient above, so that the aorta and pulmonary artery conducted into a common ventricle. During life the child was constantly blue, and extremely sensible to cold; he laboured under frequent attacks of dyspnœa, with violent action of the heart and pulsation in the jugular veins: he died with symptoms of cerebral congestion.

5. *Tubercle of the Testis.*—Mr. Cusack said, that for some time back he had been making observations on a certain form of disease of the testicle, with which, perhaps, many of the members were familiar. It was formerly looked upon as one of the forms or stages of the venereal disease, and may be regarded as a remote effect of the disease, or of the abuse of mercury. It consists in an enlargement of the testicle, more or less chronic in its nature, and sometimes accompanied by an effusion of fluid into the tunica vaginalis. It is most commonly observed in persons who have had affections of the bones, ozœna, and sloughing ulcers of the throat; but Mr. Cusack has frequently met

with it in persons who have merely had primary symptoms for which mercury had been employed, and who presented no traces of disease, except an enlargement of the testicle, with an effusion of fluid into the tunica vaginalis. The specimen exhibited by Mr. Cusack was taken from the body of a person, who died of tubercular disease. Mr. Cusack, however, was not led to conclude from this, that the state of disease, which the testicle exhibited, was to be attributable to the affection of which the patient died, namely, the general development of tubercle in the substance of the lungs. The reason assigned by Mr. Cusack was this: on examining the testicles, one of them presented the ordinary healthy appearance; the tubuli seminiferi were quite distinct, and there was no filling up of the tissue between them. The other testicle appeared to be divided into two portions; the upper presented the appearance of a uniform structure, and seemed to be, as it were, separated from the lower, in which the tubuli were still distinct. On further examination, a small tubercle was found in the more healthy part, and the question was, whether the development of tubercle in this situation, was referrible to the general disease, or to some previous disease, through which the testicle had passed. Mr. Cusack was inclined to adopt the latter opinion. In the first place there was adhesion between the two serous surfaces of the tunica vaginalis, and this was a proof of the existence of inflammation. In most of the cases which had come under Mr. Cusack's notice, these adhesions existed, and were the result of previous inflammation of the testicle, extending to the sac. From this, Mr. Cusack was led to think, that the presence of tubercle in the testis, was to be referred not to any tubercular affection of the system, but to the disease through which the testicle had previously passed. In the next place, the whitish appearance of the testicle, when a section was made through it, was the remains of that peculiar condition, which the organ assumes after acute inflammation. There is an almost complete obliteration of the tubuli seminiferi; the effused lymph consolidates the whole into one mass, and then nature endeavours to restore the natural state of the parts afterwards. Mr. Cusack said he would also submit for inspection a testicle, taken from the body of a person, who had not exhibited any symptom of venereal, or of tubercular disease, for he had examined his body carefully. In this instance the testicle presented the same appearance as in the former instance. Mr. Cusack exhibited another testicle which had been removed from the body of a patient, who died with tubercular disease of the lung. It was the only specimen of the kind he was in possession of, but he had twenty-five or thirty of the other description, in which the disease of the testicle did not coexist with a scrofulous taint in the constitution. The last specimen exhibited by him was one taken from a person who had passed through all the stages of a confirmed lues, and had died quite worn out. He had, in addition to loss of the bones of the nose and caries of the tibia, disease of the larynx and epiglottis, and furnished a remarkable specimen of what has been termed, syphilitic cachexy. Mr. Cusack concluded by observing, that he was inclined to look upon the affection of the

testicle of which he had exhibited specimens, not as common scrofulous tubercle, but referrible to the previous state of inflammation, through which the organ itself has passed. (*Museum, Park-street.*)

6. *Cavity on the Surface of the Lungs.*—Dr. Stokes exhibited a preparation of a large tubercular excavation close to the surface of the lung; its parietes were remarkably thin; it was covered in front merely by the pleura. The patient, from whom the preparation was taken, was admitted into the Meath Hospital, labouring under phthisis, in a very advanced stage: he presented the usual signs of a cavity in the upper part of the right lung. In addition to these, there was a morbidly clear resonance of the anterior parts of the chest, from the third rib downwards, and whenever the patient inspired, there was a remarkable falling in of the third intercostal space, but on expiration it flapped out, and thus the motions of the intercostal space followed the inspiration and expiration. Dr. Stokes remarked, that this sign, as far as one case went, indicated the existence of a cavity of large size, with thin parietes, and lying close to the surface of the lung.

Dr. Stokes observed, that the specimen before him was one of very great interest, as illustrating the phenomena of pneumothorax. Among the various instances of pneumo-thorax from fistula of the lung, two sets of cases may be observed. In one class the side becomes greatly distended with air, and returns a very clear sound on percussion. In the other class, although there is some air in the cavity of the pleura, there is no tension of the side, nor is there the morbidly clear sound on percussion observed in the former class of cases. Dr. Stokes said, that on a former occasion he had stated that this phenomenon depended on the nature of the opening in the lung. There are, in fact, two distinct kinds of openings; in one class the opening is direct and permanently patent, the air can pass in and out of the cavity of the pleura, and there is consequently no accumulation or increased pressure. In the other the opening is valvular, and the transit of air much more difficult. It can enter the cavity of the pleura only when the chest is greatly expanded, and from the nature of the opening it can scarcely get exit, so that during the act of expiration the chest is kept at its maximum of tension. This is of importance, not merely as a diagnostic sign, but also with reference to prognosis; for Dr. Stokes had found that cases with valvular opening were always very rapid in their course, and terminated fatally in a short time, while in cases with a direct opening, the progress of the disease was slow, and the patient lived a long time. Dr. Stokes knew one gentleman who had lived eighteen months after the phenomena of pneumo-thorax had appeared, and even enjoyed tolerably good health, except towards the latter stage of the disease. The reason of this is, that when the patient recovers from the shock given to the system by the occurrence of pneumothorax and fistula of the lung, if the opening be direct, the air can pass in and out of the cavity of the pleura with facility, there is no increased pressure on the lung, and the patient can breathe with comparative ease. Again, in many instances where there is tubercular

deposition going on in the lung, the occurrence of a new disease seems to suspend for some time the development of tubercle. But in cases where the opening is valvular, the patient's distress is greatly increased by the tension of the chest; the air not being able to find exit, distends the chest, and presses upon the lung, while at the same time the quantity of liquid effusion in the cavity of the pleura increases, and adds to the patient's sufferings. The mediastinum is compressed, the liver pushed downwards, the heart dislocated, and the patient ultimately dies of asphyxia. In the case Dr. Stokes detailed, the patient came into hospital with symptoms of laryngeal phthisis, and with the usual signs of tubercle in the upper part of the right lung. He continued in this state for some time without any remarkable change, until one night when he was suddenly seized with difficulty of breathing, and died in four days afterwards, the phenomena of pneumo-thorax being constant from the occurrence of dyspnoea up to the period of death. In this case the tension of the chest was extreme, and the patient's sufferings very great, although the amount of liquid effusion was but small. On examination after death, the opening in the lung was found to be valvular. A fold of membrane lay over the opening so as to form a complete valve, and close it in such a manner that air once effused into the cavity of the pleura could never escape. Dr. Stokes observed, that it would be easy to conceive, that when this valve closed the opening into the lung during expiration, while it permitted fresh portions to enter during inspiration, there must be a very tense state of that side of the chest, and that the pressure on the parietes of the chest and lung must be increased by that of the liquid effusion. Hence it occurred, that on puncturing the chest after death, the air rushed out with considerable force, while in cases of direct opening no such occurrence takes place.

7. *Intra Capsular Fracture of the Neck of the Femur, united by Bone.*—Mr. Adams exhibited a preparation of the head and neck of the femur, shewing a transverse intra-capsular fracture; the head of the bone was closely approximated to the posterior intertrochanteric line, while anteriorly, a well marked ridge of bony fragments constituting the upper extremity of the lower fragment of the cervix, shewed the seat of the displacement and of the union of the fragments; a section of the bone had been made, and one portion subjected to maceration and boiling, but without affecting the bony union; the head and shaft appeared to be mutually impacted into each other: by far the greater portion of the cervix was absorbed, the line of union was serrated and immoveable, and the cells of the head and substance of the shaft communicated freely in all places, except where the thin line of compact tissue here and there pointed out the seat of the welding together of the remaining portions of the head and neck of the femur. The patient was *æt.* 70: on the 1st of August, 1837, while walking across his ward, he fell on his right side, and was unable to rise; upon being carried to his bed, the limb was found everted, and half an inch shorter than the other; it was placed and preserved in a semiflexed position on pillows: five weeks after the accident he was able to put the heel of the

injured limb to the ground. Upon the 30th September he was visited by Mr. Smith, who, from the history and symptoms, gave his opinion, that the case was one of fracture of the neck of the femur, with impaction of the superior fragment into the cancellated tissue of the bone. The man survived the accident one year and nearly ten months; he spent the greater part of that time in bed, but was able, though unwilling, to walk; he died of bronchitis, May 25th, 1839. (*Museum, Richmond Hospital.*)

8. *Œdema of the Larynx*.—Mr. Adams also exhibited a specimen of œdema of the larynx, taken from the body of a man, who for twelve months previous to his death had suffered from œdema of the lower extremities; about a week since, the scrotum and penis became œdematous, the eyelids began to swell, and he got symptoms of effusion into the chest. The day before his death, he was attacked with swelling of his neck, which became quite cylindrical, and respiration was greatly impeded. On the following morning, while endeavouring to swallow his breakfast, he fell back suddenly and expired. The cause of sudden death was found to be seated in the larynx, the submucous tissue about the glottis was distended with a yellowish serous effusion. It afforded a good specimen of the serous effusion of Bayle. (*Museum, Richmond Hospital.*)

MEDICAL INTELLIGENCE.

Comparative View of the State of Pharmacy in Norway and the United States.—The materials from whence we have derived the facts stated in the following article, are chiefly, a Report on the Organization of Pharmacy in Norway, by M. Ch. Martin, D. M., addressed to the Minister of Public Instruction in France; and a paper published in the American Journal of Pharmacy, by Mr. W. Fisher, on the State of Pharmacy in the United States. These documents are principally interesting, in so far as they exhibit the working of two diametrically opposed systems of legislation; they are deserving of profound deliberation; but to derive from them their possible value, it is necessary to contemplate, in relation to them, the respective habits, prejudices, and institutions of both nations.

Every thing in regard to pharmacy in Norway is regulated by authority: the practice of medicine and that of pharmacy are completely separated in the large towns; but in the smaller villages, a class of general practitioners is permitted. However, to enable the pharmaciens to exist, by the sole practice of their art, no unlicensed person is permitted to vend medicines, under a penalty, for the first offence, of from a month to six weeks' imprisonment, and to pay the expenses of the process.

Notwithstanding, it was found necessary, according to the Report, "to avoid the consequences, equally fatal to the pharmacien, of finding it impossible to live honourably by the exercise of his

profession, and to the invalid of not being able to rely on the good qualities of the medicines furnished to him; the numbers of shops is limited in Norway, as it is in Denmark."

The principle is established, that there shall be but one pharmacy to 10,000 souls. There are consequently but thirty-five pharmacies in the kingdom; there are eight in the cities of Christiana, Drontheim, Bergen, and Christiansand; and the remainder in the small cities and villages. The privilege of a pharmacy, once accorded by the State, cannot be taken away on any pretext; it becomes the property of the incumbent. However, if he is proved guilty of fraud, of vending sophisticated drugs, or shews himself to be unworthy to exercise his profession, the privilege can be taken from him, by the judgment of a competent tribunal; but it is not therefore destroyed; it must be immediately afforded to some other pharmacien, who is found to reunite all the conditions of capacity required by the laws. Except in these rare exceptional cases, the incumbent transmits his privilege to one of his pupils, his son, or a stranger.

The value of a pharmacy is very great; it varies from 50,000 to 100,000 francs. If the incumbent has a son whom he destines to succeed him, he has the right, in case of death, to cause the pharmacy to be conducted provisionally by a proper person, who returns the establishment to the son, when the latter has acquired the necessary qualifications.

These privileges, according to the reporter, would give rise to intolerable abuses, if it was lawful for pharmaciens themselves to fix the price of their medicines; but it is sought by the establishment of a tariff to obtain the advantages which may result from this arrangement, avoiding its inconveniences. This tariff follows the oscillation of the price of drugs, and accords to pharmaciens a benefit of $128\frac{1}{2}$ per cent. The ordonnance which fixed this profit owes its origin to the epoch at which Norway again became united to Denmark.

To carry this tariff into execution, the different substances contained in a pharmacy have been divided into *simple drugs*, and *compound medicines*. The *simple drugs* have been divided into three categories.

1st. Exotic drugs, which are to be found in the country, in sufficient quantities.

2nd. Products indigenous to Norway.

3rd. Exotic drugs, or substances which it is necessary specially to procure from Hamburg, London, or Amsterdam.

The tariff of the first category is fixed by the price current at Christiana, there being added to this first cost $128\frac{1}{2}$ per cent.;—thus what is bought for seven francs, is sold for sixteen francs.

The second category comprehends indigenous herbs, roots, and barks. The value at the time of collection is taken as the standard; and allowance is then made for loss during desiccation. Thus if a pound of fresh herbs cost four skellings, (a skelling is equivalent to a halfpenny,) and that in drying, five pounds were

reduced to one, the price would thus rise to twenty skellings, to which $128\frac{1}{7}$ being added, the selling price would be $45\frac{1}{7}$ skellings.

The prices of exotic medicines, which form the third category, regulate themselves according to their value at Hamburgh; to which is added, $33\frac{1}{2}$ per cent. for freight, and the usual profit.

Compound Medicines are also divided into categories.

1st. Those which are modified by mechanical means; trituration, pulverization, &c.

2nd. Pharmaceutical preparations, such as tinctures, distilled waters, extracts, &c.

3rd. Chemical preparations.

For medicines of the first class, certain additions are made, partly for loss, and partly for manipulation. Thus bark, whose original cost was 16 skellings, when bruised is raised to 20 skellings, powdered to 26 skellings.

As to the second class, allowance is made for every possible loss; thus in alcoholic distillation, one-tenth is subtracted, and consequently nine ounces of product are sold for the price of ten. Again, the trouble of manipulating is compensated for, according to a similar scale. Thus a skelling an ounce is allowed for infusing and boiling; 16 skellings a pound for distilling waters, but if glass vessels are employed, half as much more; in making extracts, the cost of manipulating is calculated from the quantity of water employed, for which 16 skellings a pound is allowed; for making liniments, ointments, plasters, conserves, electuaries, a skelling an ounce is allowed, and ten per cent. for loss.

We must now say a few words with respect to the allowance made for certain chemical and pharmaceutical processes. These are, of course, added up in the value of a compound medicine.

Boiling in a metallic vessel,	6	skellings	per lb.
Evaporating and crystallizing,	8	do.	do.
Distilling, where the product is { little volatile,	8	do.	per oz.
	{ very volatile,	6	do. do.
Dissolving salts, { in the cold,	1	do.	do.
	{ with a gentle heat,	2	do. do.
	{ with a high temperature,	4	do. do.
Saturating a base with an acid,	1	do.	do.
Calcination, { in a gentle fire,	2	do.	do.
	{ in a violent fire,	8	do. do.
Precipitation,	6	do.	do.
Fusing the bulk of an orange,	32 to 34	skellings.	
Subliming, { in a vessel, from 1 to 4 ounces,	4	do.	
	{ in do., from 4 to 15 ounces,	64	do.
Fabricating gases, { in the cold,	{ for 27 {	2	do.
	{ with a gentle heat, { cubic {	3	do.
	{ with a red heat, { inches, {	6	do.

We shall now shew the working of the above tariff by an example taken from a compound medicine, commonly kept in the pharmacies.

The calculation of the price of compounding an ordinary prescription becomes, of course, much more complicated.

Aromatic Vinegar.	Quantities used.	Price per Ounce.	Price of Quantity used.
Sliced wormwood, .	0,023 oz.	2 skellings,	0,046 skellings.
Sweet scented grass,	0,023 „	2,5 „	0,058 „
Sage,	0,023 „	6 „	0,078 „
Peppermint, . . .	0,023 „	6,5 „	0,090 „
Cinnamon, . . .	0,006 „	8 „	0,048 „
Cloves,	0,003 „	10 „	0,030 „
Nutmegs,	0,003 „	40 „	0,120 „
Vinegar,	1,104 „	1,5 „	1,656 „
			2,126
For digesting, .			1,104
Price of an ounce of Aromatic Vinegar, . .			3,230 skellings.

The appearance of homœopathic medicine, by remarkably diminishing, during some years, the quantity of medicines required from pharmaciens, made it necessary to modify these regulations ; and accordingly, on the 13th of September, 1830, there was added a further profit of $16\frac{2}{3}$ per cent. ; and thus, a medicine bought for 100 in Hamburgh, was sold in Norway for 355.

“ Notwithstanding,” says the reporter, “ the benefits which accrue, both to the public and the profession, from these regulations, they give rise, it must be acknowledged, to some grave abuses.” Smuggling, it appears, is very common, as well as the secret vending of medicines. The pharmaciens, too, are discontented, and prefer the Prussian tariff, which affords a high profit on cheap and common substances, but a small one on expensive drugs !” Further on, M. Martin states, “ Without doubt, some abuses exist here, as well as elsewhere, but they are at a minimum, in comparison with those we see every day practised in France.”

The pharmaciens in Norway possess certain privileges, beside those already mentioned : they have the sole right of furnishing (with 20 per cent. abatement) medicines to the general practitioners of villages ; they also furnish the hospitals and dispensaries, at the tariff prices, without any abatement. When there are two pharmaciens in a city, each alternately furnishes medicines to the medical charities, during the year.

The pharmacies are regularly inspected ; the incumbent is subject to heavy fines for having inert or adulterated medicines, also for giving medicines without a physician's order. The fines are 500 francs for the first offence, and 1000 francs for the second : he is also prohibited from having secret medicines.

The regulations with respect to pharmaceutical education are admitted by the reporter to be very indifferent. A boy from school, without any initiatory examination, is permitted to enter a shop; after an undefined period, he is allowed to execute prescriptions, under the surveillance of an assistant; after a time, equally uncertain, he obtains what is called a "scherbrief" from the pharmacien, and, furnished with this, he undergoes an examination before the physician of the district, who gives him a certificate, declaring him capable of performing the functions of an assistant in a pharmacy. Afterwards, whenever he thinks fit, or has a prospect of obtaining a pharmacy, he goes to Christiana, and undergoes an examination before a jury. This latter appears to be a very proper examination, being partly written, partly verbal, and partly practical.—So much for pharmacy in Norway.

Our readers will now please turn their attention to the other side of the picture,—the state of pharmacy in the United States. Instead of laws without number, and interminable tariffs, there is only one law in Carolina and Georgia, and a parallel one in New York, controlling the practice of pharmacy. The first requires any person about to open a pharmacy, to undergo an examination before a council of censors, but the said council having never been appointed, the law is inoperative. The second, passed in 1832, and which came into force on the 1st of January, 1835, is to the effect, that no one is to open a pharmacy in the State of New York who has not obtained a diploma from some legally qualified college, or school of medicine. There is another provision, that any apothecary vending arsenic, prussic acid, &c., is to write thereon "Poison," in legible characters. Throughout the other States of the Union, the regulation of this Profession is left to the emulation of its members, and to the influence of public opinion, which in that country rules every thing. What are the results?

Every where through the States, the physician is ceasing to dispense his own medicines. In the country parts, the re-union of different branches of practice still continues, but in cities it is abandoned, and the division of labour is becoming more and more firmly established. The pharmacies, also, which were formerly confounded with general drug and commercial marts, are rapidly assuming a purer character; pharmacy, properly so called, is now separated from the sale of drugs. The promiscuous sale of colours, dyes, drugs, and the compounding of medicines, are now considered incompatible with the order, propriety, and safety, which is known to be necessary in these establishments for fixing public confidence, and acquiring reputation. These happy reforms have been produced solely by the voluntary and praiseworthy efforts of apothecaries themselves, jealous of ameliorating their condition, and of elevating their occupation in the public esteem to the rank of a liberal Profession.

Moreover, colleges of pharmacy have been established at Philadelphia, and New York, chairs of chemistry, materia medica, and botany, have been created. The College of Philadelphia possesses a complete chemical apparatus; a cabinet of specimens of materia

medica ; an excellent library of choice works on pharmacy, and the accessory sciences. It was instituted in 1820 by a voluntary association of the apothecaries of the city. The candidates for its license must have served an apprenticeship of four years, and obtain a certificate for moral character, assiduity, and intelligence. They must undergo an examination, write and defend a thesis, and produce evidence of having attended, at least, two courses of lectures on each of the subjects already mentioned. Their theses are in general, it is said, "honourable productions," shewing an intimate acquaintance with that most delicate and minute branch of chemical science—analysis. Now, when it is taken into consideration that the *forty* yearly candidates for this difficultly obtained diploma can have no other end in view, inasmuch as they need not absolutely acquire it, but the noble ambition of possessing a testimony of their talents and information, we must conclude with Mr. Fisher, that the United States will soon possess a corps of distinguished pharmaciens, animated by an honourable emulation.

The College of Philadelphia possesses a spacious and elegant building, where the members hold their meetings, and an amphitheatre for lectures. But we must not forget the "American Journal of Pharmacy," first instituted in 1829, under the name of the "Journal of the College of Pharmacy of Philadelphia." This Journal, whose eminence may be estimated from the fact of its being regularly exchanged for the French and German Journals, published on the same subjects, is under the direction of the College of Pharmacy of Philadelphia, but has impressed on it a national character by the appointment of corresponding collaborateurs, in the principal cities. It contains many excellent memoirs, which have been reprinted and translated in foreign lands.

This article would be incomplete, if we did not exhibit the *facility!* with which restrictive laws can be put in force. We are fortunately enabled to do this, to a certain extent, by adding the following analysis of a communication made to the Society of Pharmacy in Paris, in the year 1837, by a committee appointed for the prosecution of delinquents.

It appears, that in the year 1832, thirty Parisian pharmaciens united for the purpose of prosecuting unlicensed drug-sellers. They obtained decrees at the Court Royal of Paris, and the Court Royal of Rouen, and these decrees were confirmed by the Chambers. However, violations of the law still continued ; and to use the words of the Report, "unfortunately since this period, and too frequently, they have been obliged to enforce against a great number the highest penalty, (imprisonment ;) and now this penalty has ceased to be efficacious, and the tribunals, by the feebleness of the laws !!! are become powerless." The Report proceeds to state, that seventy-nine Parisian pharmaciens now form the association, (verily, the grievance must be a sore one,) sixteen delinquents were prosecuted, and twelve were condemned in January, 1837, and four in the February following, to imprisonment and heavy fines, varying from four thousand francs to twenty-five francs.

JOHN ALDRIDGE.

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PART I.
ORIGINAL COMMUNICATIONS.

ART. XVI.—*Sketch of the Origin and Progress of Asiatic Cholera.* By ROBERT J. GRAVES, M.D.

WHILE the art of navigation was in its infancy, and communication by land between distant countries unfrequent and insecure, the different races and families of mankind who dwell far asunder on the earth's surface, were necessarily unacquainted with the appearance of new, or the existence of remarkable diseases amongst each other, and consequently, that department of medical science which may with propriety be termed, the Geography of Diseases, remained uncultivated. Now, however, we approach a new era, when the means of intercourse between the most distant nations have been so facilitated by the aid of an improved system of navigation, a commerce almost universal, and the daily increasing efficiency of steam power, that we may indulge in the rational hope of seeing the sciences studied after a new method, which will embrace within the range of ob-

servation, not merely the phenomena occurring in a single district or country, but those which take place over the whole surface of the globe. Already have the enlightened efforts of our own University, and the genius of one of its Professors, prompted the rulers of many kingdoms to join in an alliance destined to establish magnetic observatories in distant regions, so as to make the globe of the earth itself a subject of extended experiment; the philosophers of the new world have combined with those of the old, to examine simultaneously meteorological phenomena, and already have the records preserved by observers at sea and land, revealed the hitherto mysterious course of storms, and enabled us to map out the extent and direction of the shocks of earthquakes. When we investigate the physical changes which occur on our planet, we are encouraged to repeat and multiply observations, in the hope of discovering general laws, whose application will enable us to explain the past and predict the future. But the surface of the earth abounds with beings in whom the creative powers of life display an order of phenomena more complicated and refined than anything existing in unorganized matter. But for this very reason, and on account of this superiority conferred on organized matter through the agency of vitality, each being thus animated is governed by laws which seem incapable of extension even to other living creatures of the same species; and consequently we are led to expect an individuality, an insulation, among animals, which will prevent them from exhibiting changes occurring simultaneously among great numbers, and capable of being traced to the operation of general laws. A closer examination, however, proves that animals and plants are subject to the operation of physical agencies which act upon numbers of individuals at the same time, and thus give rise to great varieties of diseases. Such diseases should be made a special object of study; many of them are, as it were, fixed, stationary, and confined to certain countries and districts.

Thus the goitres, the *tumidum sub Alpibus guttur*, has from the earliest times been endemic in the valley of the Rhone, and other parts of Switzerland ; modern travellers have observed it in certain parts of South America, and in *Kemaon*, a sub-alpine department of Hindostan. Agues, typhus, yellow-fever, elephantiasis, berri-berri, Guinea-worm, yaws, Egyptian ophthalmia, are chiefly confined to the inhabitants of certain districts, and with a host of other complaints, would afford ample materials for the geography of fixed diseases.

On the other hand, there are affections of men and animals which travel from nation to nation, and tribe to tribe ; sometimes these moving epidemics progress with such rapidity, that they speedily migrate over the whole earth ; at other times they creep along with a slow and stealthy step, but their journey is continued year after year, until they have travelled round the world. The Asiatic cholera affords an example of the latter class, having been twenty years in compassing the earth ; while influenza, an example of the former, often traverses the same space in a few months. Thus, the epidemic influenza of 1830-32, existed in Australia, and was afterwards noticed in the northern hemisphere at Moscow, whence in eight months it extended to St. Petersburg, Warsaw, Frankfort, Paris, London ;* three months subsequently, it appeared in Italy, and shortly afterwards in Gibraltar. Now it is deserving of attention that this influenza travelled from Moscow to London in eight months, and to the United States of America in seven months more, and allowing something for the inaccuracy of dates, these data give its rate across the Atlantic only a little speedier than across the Continent. This forms, as we shall hereafter see, a striking contrast with the progress of cholera from Britain to Quebec, as compared with its march from Moscow to London, and is a fact of considerable weight in arguing whether cholera, like influenza, is propagated by atmospheric influences.

* British and Foreign Medical Review, No. xiii. p. 105.

The influenza of 1833, travelled much more rapidly than that of 1832, for originating in the north-east, there was but a few days' interval between its appearance in Moscow, Odessa, Alexandria, and Paris !

Influenzas differ from each other, not merely as to their rate of travelling, but as to the extent of the earth's surface which they affect. Some, as that of 1782, spread from China all over the inhabited parts of Asia, Europe, and America; while others, as for instance the great influenza of 1837, did not reach the new world at all, although it passed the equinoctial line, and was severely felt at the Cape of Good Hope and Australia. These facts are alone sufficient to stimulate our curiosity, and ought to direct the attention of philosophers as well as physicians, to the study of endemic and epidemic diseases; nor will their study be destitute of practical benefit, for were the rulers of civilized nations to bring into active operation a number of institutions, which discharging the functions of *medical observatories*, should observe and record the appearance and symptoms of epidemics, many curious facts relating to their origin and progress would be soon brought to light, and we might then perhaps be enabled to arrive at a knowledge of some general laws respecting their motions. Thus we could ascertain whether, as has been asserted, influenza always progresses from east to west, never from west to east; whether originating on one side of the equator, it often passes to the other? As the means of communication are now-a-days so rapid, it is quite possible to learn the character and the best mode of treating an epidemic disease long before its arrival amongst ourselves; we knew, for instance, the symptoms and best method of treating the influenza of 1837, several weeks before we experienced its shock, and we had for many years been familiar with the symptoms of cholera before we actually witnessed its baneful effects. I have still by me a manuscript copy of a lecture I gave at the Meath Hospital in 1826; in that lecture I accurately described, from eastern authors, the symptoms of

spasmodic cholera, and prepared the class for its future arrival in Great Britain, a prediction not my own, but derived from that illustrious philosopher, and truly excellent man, Doctor Brinkley, then President of the Royal Irish Academy.

The origin and march of the spasmodic cholera will form the subject of the remarks which I now mean to lay before this meeting. In India, or, more properly speaking, in Hindostan, the spasmodic cholera is not a new disease; partial epidemics of it have occurred at different times since that empire has been familiarly known to the English. These epidemics, however, being almost exclusively confined to the natives, comparatively circumscribed in extent, and limited in duration, did not attract much attention on the part of European writers.

“ In 1762 it prevailed very extensively in upper Hindostan, destroyed, according to Le Begue de Presle, thirty thousand negroes, and eight hundred Europeans. Dr. Paisley, in a letter from Madras in 1774, states that it was often epidemic, especially among the blacks. M. Sonnerat, in the account of his travels in India, between the years 1774 and 1781, mentions that cholera prevailed on the Coromandel coast, and at one period more particularly, assumed an epidemic and malignant character. Curtis, in his work on the Diseases of India, and Girdleston, in his essay on the Spasmodic Affections of that country, speak of an unusual prevalence of the disease during 1781 and 1782. It prevailed in the northern Circars in the early part of 1781, and in the latter end of March it affected at Gangam, a division of Bengal troops, consisting of five thousand men, who were proceeding under the command of Colonel Pearse, of the artillery, to join Sir Eyre Coote’s army on the coast. Men, previously in perfect health, dropped down by dozens, and those even less severely affected, were generally dead, or past recovery, within less than an hour. Above five hundred were admitted into the hospital in one day, and in three days, more than half the army were affected.

“ In April, 1783, it broke out at Hurdwar, on the Ganges, a spot held peculiarly sacred by the Hindoos, among a crowd of

between one and two millions of persons, assembled for the purpose of ablution in the holy stream. It is the custom of the pilgrims to repair to the bed of the river, where they pass the night with little, if any shelter. Very soon after the commencement of the ceremonies, the cholera attacked the pilgrims, and in less than eight days, is supposed to have cut off twenty thousand of them. The disease was, however, on this occasion so confined in its influence, as not to reach the village of Jawalpore, only seven miles distant.”*

In Europe no such disease as spasmodic cholera had been known; this assertion, though opposed to some authorities, may be considered as well founded, and indeed I have no doubt of its accuracy. With us spasmodic cholera is an imported disease; in Hindostan a resident endemic. What causes combined to convert a malady habitually confined to the Indian peninsula, into a disease which overshadowed the earth, sparing no nation or language, it would be useless to inquire; the subject is buried in profound obscurity: in the mean time let us hope that it will not prove a permanent addition to the nosology of every country, and that it will soon return within its former limits. It was in the spring of 1817, that the cholera of India assumed a new and more powerful character; it was then it became endowed with properties that rendered its extension steadily progressive over the earth, in spite of all the obstacles interposed by diversity of soil or climate. The disease first assumed the migratory and epidemic form in districts bordering on the Ganges, and some of its tributary rivers, at a distance varying from 80 to 150 miles from Calcutta. This took place in the spring and summer, but the date of its commencement is usually referred to the period of its outbreak at Jessore, on the 19th of August, 1817, where the epidemic was first medically observed and described by Dr. Tytler, who erroneously attributed it to the use of bad rice. Jessore is situated in the Gangetic Delta, about 100 miles north-east of Calcutta. The cholera was now

* American Cholera Gazette, p. 3.

observed in general to follow the course of the rivers, and soon arrived at Calcutta, where it commenced its ravages in September, 1817, and continued to rage during nearly the whole of 1818.

“ By the latter end of September the disease was prevailing throughout the whole province of Bengal, from the most easterly limits of Purnea, Dinagepore and Silhet, to the extreme borders of Balasore and Cuttack ; and from the mouth of the Ganges nearly to the confluence of that river with the Jumna, a space of upwards of four hundred miles in length and breadth. In this area of several thousand miles, few places escaped the invasion, and the cities of Dacca and Patna, the towns of Balasore, Burrissaul, Rungpore, and Malda suffered severely. The large and populous city of Mooshedabad, which, from extent and local position, was apparently favourably circumstanced for the attacks of the epidemic, it is remarkable, escaped with comparatively little loss, whilst all around was severely scourged.

“ During the autumn of 1817, the disease extended itself to Muzufferpore and beyond the precincts of Bengal, and appeared at Chuprah, and at the cantonment of Gazeepore ; its attacks in these places were, however confined to the towns themselves, or villages in their immediate vicinity ; the principal portion of the adjoining country, at this period, entirely escaping the disease. Early in November it attacked the grand army, then stationed at Bundlecund, a portion of the Allahabad province. This army had been assembled in anticipation of a war with the the Pindarees, and the centre division, consisting of ten thousand fighting men, and eighty thousand camp followers, was encamped on the banks of the Sinde, under the immediate command of the Marquis of Hastings. Here the cholera exercised its most destructive power. It is uncertain whether it made its first approaches on the 6th, 7th, or 8th of the month. After creeping about, however, in its wonted insidious manner for several days among the camp followers, it seemed all at once to have gained vigour, and burst forth with irresistible violence in

every direction, extending through the whole camp before the 14th of the month. Old and young, European and native, fighting men and camp followers, were alike subject to its attacks, and all equally sunk in a few hours under its pestilential influence. It was a common occurrence for sentries to be suddenly seized at their posts, and having been carried in, to have two or three successors before the two hours' duty was performed. Many of the sick died before reaching the hospitals; and even their comrades, whilst bearing them from out-posts to medical aid, sunk themselves suddenly seized with the disorder. The mortality at length became so great that there was neither time nor hands to carry off the bodies, which were thrown into the neighbouring ravines, or hastily committed to the earth on the spots where they expired, and even round the walls of the officers' tents. In the five days included between the 15th and 20th of November, the number of deaths amounted to five thousand. The natives thinking their only safety lay in flight, deserted in great numbers; and the highways and fields for many miles round were strewed with the bodies of those who had left the camp with the disease upon them, and speedily sank under its exhausting influence. The camp being now cumbered with the sick, the Marquis of Hastings determined to seek a purer air for the recovery of his sick. Although every means was put in requisition for their removal, a part was necessarily left behind. 'And as many who left the carts, pressed by the sudden calls of the disease, were unable to rise again, and hundreds dropped down during every subsequent day's advance, and covered the roads with dead and dying, the ground of encampment, and line of march, presented the appearance of a field of battle, and of the track of an army retreating under every circumstance of discomfiture and distress.*' The exact mortality could not be ascertained, but it appears that of the fighting men seven hundred and sixty-four fell victims, and it was estimated

* Bengal Report, pp. 12-15.

that about eight thousand camp followers, or one-tenth of the whole, were cut off. On arriving at the high and dry banks of the Betwah at Erich, the army soon got rid of the pestilence and met with returning health.

“During December the disease appears to have every where abated, and in January of 1818, to have become nearly extinct. Towards the latter end of February it however revived with great force, and before the close of the year, the whole peninsula of India, from Silhet on the east to Bombay on the west, and from Deyrah on the north to Cape Comorin on the south, had suffered from its ravages.”*

The ravages of the disease were much facilitated and increased by the superstition of the people, who, in obedience to the Brahmins, collected in prodigious multitudes on pilgrimages to certain favourite shrines, where they prayed for the cessation of what they were taught to believe the cause of the epidemic, viz. a violent and protracted battle between the god and goddess answerable for the tranquillity and happiness of that part of the world.

During the year 1818 the cholera pursued a three-fold route. First, ascending the Ganges and the Juhmna, it reached the northern provinces of Hindostan, but was there checked in its progress for several years by the Nepaulese mountains, and finally entirely arrested by the Himalaya range. This is easily accounted for by the thinness of the population in these situations, and the little intercourse which takes place between the mountainous districts and the lower regions. Cholera did not in India attain to an elevation beyond six thousand feet above the level of the sea; in June, 1818, it had reached the range of mountains between Nepaul and Hindostan; it was at *Schaurapoor*, many hundred miles to the north-west, in October; and before the end of the year had ravaged nearly all the numerous cities and villages situated in the vast tract of country watered

* American Cholera Gazette, p. 19.

by the Ganges, the Juhmna, and their tributaries. This was one of the most thickly inhabited parts of India, and the destruction of life was awful.

The second route was southward along the coast from one sea-port to another, until it reached Madras on the 24th of October, 1818. Here, at the very onset of the disease, twenty medical men were attacked, of whom thirteen died.

Sadras, Pondicherry, and the whole Carnatic, were affected during the succeeding year; but even in December, 1818, it had reached Jaffnapatam, the most northern town of Ceylon, having passed thither after travelling along the whole coast of Coromandel. On the 10th January, 1819, it broke out in Colombo, and produced dreadful devastation on the western coast of Ceylon; the disease became exhausted there, but at the same moment burst forth with renewed vigour in Candi, the capital, 2500 feet above the level of the sea. The cholera did not arrive at the east coast of Ceylon until 1820, when it appeared imported, as was said, into Trinkamalay by the flag ship *Leander*. The epidemic was brought to the western coast of the Indian peninsula, partly by sea round Cape Comorin, and partly by the great over-land lines of communication which connect the Presidency of Bombay with the Presidencies of Madras and Bengal.

It first showed itself at Bombay on the 9th of August, 1820, and in that Presidency carried off 150,000 persons.

The third route of cholera in India I have already referred to, it was across the peninsula from the east coast to the west; it came by Nagpoor, Ellishpoor, Aurungabad, Siroor, and Poonah, to the Bombay coast, and was introduced either by troops or travellers.

From Ceylon the disease went to the Mauritius, and the Isle of France; whither it was said to have been imported on the 29th October, 1819. The distance thus traversed at one spring was three thousand miles. Thence it soon passed to the Isle of Bourbon; and in the year 1820 to the east coast of Africa at

Zanguebar. It is remarkable that it never reached the Cape of Good Hope, where the strictest quarantine was observed.

The following are the dates of its arrival in the subjoined places :—

Burmese empire generally ; Aracan, Ava, 1819.

Malacca, 1818.

Sumatra, 1819.

Java, Batavia, (fearful,) 1821.

Madura ; Macassar ; after Batavia.

Amboina, in Moluccas, 1823.

Amboina was the farthest south-easterly point it attained to.

The disease visited Borneo and Celebes ; and in 1820 broke out with extraordinary violence in the Philippine Islands, principally at Manilla, where the natives, misled by the idea that they were the victims of poison administered by the Europeans and Chinese, rose *en masse*, and were not put down until 15,000 lives had been sacrificed in the contest. Similar manifestations of feeling led to some loss of life even in Petersburg and Paris, when cholera reached those cities. The same suspicions agitated the inhabitants of Europe during the ravages of the black death in the fourteenth century, when the Jews were slain in great numbers as authors of the plague. In Great Britain I am not aware that any such insane popular ideas were manifested when cholera appeared. In Ireland nothing of the sort was displayed ; and barbarous, cruel, and uneducated, as we are said to be, the visitation was in no country met with greater intrepidity and resignation, than in our native land. When a city or town was attacked in Ireland, we never witnessed the flight of the better classes ; there was neither migration into the country, nor desertion of their poorer fellow-citizens. No ; I record the fact with pride, every one remained, every one was ready to do his duty and abide in his place until the plague was stayed. In Dublin, and generally throughout Ireland, the members of the medical Profession, and the public at large, believed the malady to be contagious, and yet the sick were never abandoned

by their friends in private houses, nor in the least neglected in the hospitals.

In 1819 the cholera appeared in Siam, Bangkok, Tonkin, Cochin-china, and caused immense loss of life in Cambodia. In 1810 it arrived at Macao, and was said to have been imported by some ships; thence it travelled to Canton in China, and coming to Nanking in 1820, penetrated as far as Peking in 1821. In China the disease proved particularly fatal on account of the denseness of the population of the Celestial Empire.

So far we have followed the cholera chiefly southward and eastward in the first instance, but afterwards far to the north; in this part of its course it passed 10° to the south of the line, and then resuming a northerly direction, went on to Peking, in latitude 40° north. Even this portion of its progress leads forcibly to the conclusion, that it followed the track of commerce, whether by land or sea, and was not dependant for propagation on mere local influences, or climate. *There is a popular idea current, that its course was westward; such was the case in Europe, but in most of Asia it was eastward.*

I have already said that the Himalaya range opposed the progress of the disease northward from Hindostan, and that the highest altitude it attained to was six thousand feet. With respect to this latter point, I learn from my friend Captain Meredith of the 13th Regiment, and who has just returned from India, that it broke out in the medical depot at Landour in 1838, for the first time, at a height of eight thousand feet above the level of the sea. It is worthy of remark that cholera did not come to New Holland, although it was in several islands, as Borneo, and Celebes, to the north of Australia; but it is to be noted, that there is little or no communication between them and the settled portions of New Holland.

Let us now trace its progress westward from Hindostan. The general belief in Persia is, that the disease was brought in ships from Bombay to Mascate, Bonderabassi and Bassorah, in

which places it appeared nearly at the same period, in spring, 1821.

From Bassorah and Bender-abassi, the epidemic spread in a well defined and marked manner, along the rivers and routes most frequented by commercial travellers.

Thus from Bassorah it crept up the Euphrates and Tigris; and in August, 1821, was at Bagdad, where it carried off great numbers of the Persian army then besieging that city. Along the Euphrates it proceeded to the ruins of Babylon, and by the great route of the caravans across the Desert, it arrived at Aleppo. Here it did not commit great ravages, and ceased in the following December; but afterwards extended to different towns in Asia Minor, as Mosul, Merdin, Darbeker. At Alexandretta, situated on the Gulf of Scanderoon, it did not arrive until 1823. It is strange that cholera did not continue very long in Asia Minor or Syria, and did not at that period penetrate into Egypt.

From Bender-abassi in Persia, cholera travelled along the great mercantile road to Shiras in August, 1821; and thence to Yezd, where it appeared towards the end of September, but on the approach of winter lay dormant until spring, 1822, when it again showed itself, and spread north-westward, committing the greatest ravages in every town and village situated on the great caravan road. Tauris, Korbia, Ardabil, and the provinces of Kalkhai, Masinderan, and Gilan, (on the Caspian,) were soon infected. In most of these places it seemed to cease for a time, but reappeared in the middle of 1823; and travelling along the Persian seaports of the Caspian, it reached the province of Shirwan, then lately ceded to Russia. Here it ascended the river Cur, and progressed along the high roads to the fortress Baku; and on the 21st September, 1823, Astrachan was attacked. In June, 1823, cholera showed itself in the neighbourhood of Laodicea and Antioch, (modern names,) and then spread in two directions along the coast of the Medi-

terranean, but disappeared again both there and on the coast of the Caspian Sea.

On the whole then, the epidemic, from its commencement in 1817, till the end of 1823, had travelled over ninety degrees of longitude, and sixty-six degrees of latitude, viz. from the Philippine Islands to the coast of Asia Minor, and from the island of Bourbon, to Astrachan, and to the Caspian Sea.

It is very remarkable that cholera did not come to Europe by way of Asia Minor; this circumstance may perhaps be explained by the accident of its not having infected Smyrna, the chief seaport of communication between Asia Minor and Europe. Had Egypt likewise been then attacked by cholera, it is doubtful whether Europe would have been so long spared. Be this as it may, from the end of 1823, until its out-break at Oronburgh in 1829, cholera seemed to halt on the very confines of Europe, so that we may consider the years from 1817 to 1823, as constituting the first period in the progress of this epidemic.

But although the cholera ceased to attract much attention in Europe during the interval which elapsed between 1823 and 1829, yet we are not on that account to conclude that it lay entirely dormant, for we find it continued its ravages in its original seat, India, and extended itself from Asia Minor, Persia, and China, through the vast regions of Tartary and Chinese Tartary.

The thinness of the population in these half desert regions, may be the reason why the progress of the disease through them was at once so uncertain and so slow; the want of frequent communication between even neighbouring districts, may have baffled for a time the march of the pestilence, and may have occasioned its remarkably slow progress towards the Russian frontier. Certain it is that this march in Persia, Tartary, Mongolia, and Thibet, countries absolutely destitute of regular roads, formed a striking contrast with its rapid transmission

through more populous and highly cultivated countries, or its still quicker passage from one maritime nation to another, when connected by a constant trade, as from Germany to England, from England to Canada, and from the East Indies to the Isle of France. In the latter cases the epidemic sprung from one country to another ; *but it is remarkable that it never traversed the ocean at a rate exceeding that of ships.*

We next come to the second period of the history of cholera, when it broke out at Orenburgh, in August, 1829, where it raged with great violence, spreading throughout the whole of that Russian province; while the disease, after long lingering in the north of Persia assumed, in 1829, an increased energy in that kingdom, from whose northern portions it spread along the western coast of the Caspian, arriving at Salian, and the province Shirwan in June, 1830; and thus spreading to Baku, Kuba, and Sheki, in Chomath Talisch, and in the district Elizabethpol. From this the epidemic pursued a two-fold route; the one following the Kura, upwards, led to Tiflis, where the mortality reached five thousand; and thence to the provinces between the Black Sea and the Caspian, until it a second time came to Astrachan, and proved much more fatal in that city than in 1823, now counting more than eight thousand victims.

From Astrachan the progress of the cholera up the Wolga or Volga, was very remarkable, as it spread from town to town on that river, in the direct route of intercourse and traffic. I here may remark, that whenever cholera travels up the highest mountain passes, as in India, or traverses the ocean, as to the Isle of Bourbon, or accompanies the caravan across the desert, as when it arrived at Mecca and Medina, or when it ascends rivers, making the towns on its banks the successive stages of its journey; in all such cases, cholera, I say, seems regulated by no common physical circumstances, except human traffic and human intercourse; for in other things these lines or routes, differ remarkably from each other. But to follow its ascent of the Volga: in 1830, in August, it came to Saratow, and shortly after to

Kasan, Nischnei Nivogrood, Kostroma, Jarislaw, and so on to the circle Tischwin, in the government Nowgorod, where it was only 250 versts distant from Petersburg, and where it attained for that year to its highest northern limit.

From the country between the Caspian and Black Sea, it spread through the Caucasus to the Don, which it ascended, while it coasted the Black Sea to Cherson and Odessa, in September and October, 1830.

The stream of cholera which entered Russia from the northern provinces of Persia, as it may be seen from the foregoing account, soon formed a junction with that which flowed from Tartary through Orenburgh.

In the middle of September, 1830, the disease appeared in the government of Moscow; and on the 20th of September in the capital itself, and did not cease until the end of the following March. In Moscow a severe frost and snow set in towards the end of November, without in the least diminishing the diffusion or the intensity of cholera. Its unabated continuance throughout the whole of a Moscow winter, is a fact worthy of attention; in Moscow, according to Jahni-chen there sickened between thirty and forty per cent. of the persons who had hospital duty to perform, including physicians, nurses, &c., while of the whole population not more than three per cent. took the disease. In Dublin likewise great numbers of the hospital attendants were affected, and many died; still more were saved by the timely exhibition of remedies. It is not quite correct to affirm that cholera ceased in Moscow in March, for in the Autumn of 1831 more than one thousand cases occurred.

During the winter and spring, 1830-31, cholera spread far to the west and south, viz. to Kaluza, Tula, Pultawa, Kiew, Podolia, Bessarabia, Bulgaria, and Silistria, and through the river-provinces of the Dnieper, the Bug, and the Dniester.

In the more northerly and eastern governments, the disease had ceased, while it continued, though in a milder form, in the

provinces Nicolajaw, Crakow, Tauris, and among the Cossacks of the Black Sea. Petersburg a second time remained untouched, although the disease had arrived at Tishwin, within one hundred miles of it, an immunity to be attributed to the strict precautionary measures adopted, and the *cordon sanitaire* drawn around the capital for the protection of its inhabitants, but not of its emperor, Nicholas, who, it is but just to add, had gone to Moscow the moment he had ascertained the existence of cholera in that city, in order to exert himself in alleviating the sufferings of his subjects. The fear of infection proved no obstacle to the Czar, who zealously performed his duty on that trying occasion. The following facts relative to Russia are taken from Dr. Simpson's *brochure*.

“ *Pensa*.—From the accounts of the progress of the cholera in Russia, where the disease was so accurately observed, we shall subjoin only two cases, the first of them containing the history of the introduction of the malady into a village, in the government of Pensa, as detailed by Sir William Russell, (on what he considers to be sufficiently credible authority), in one of his letters from St. Petersburg :—[See Edinburgh Medical and Surgical Journal for February 1832, Supplemental Number, pages 173-4.] The son of an inhabitant of the village, who was coachman to a nobleman, at fifty versts distance, died of cholera. The father went to the place to collect the effects of the son, and brought home with him his clothes, which he put on and wore for a day or two after his arrival in his native village. He was shortly after seized with cholera, and died of it. Three women who had watched him in sickness, and washed his body after death, were also seized, and died of the disease. The common street of the village was then barricaded on that side which the disease had not reached, and all intercourse interdicted with the infected side. In that side of the village where the disease first broke out, upwards of 100 cases of cholera occurred, and of these, 45 died, while on the other side which had been barricaded, no case was observed.”

“*Iletsk*.—We select the following case from the official Russian Reports, as affording a remarkable instance of the transportation of cholera by exposed and infected individuals over a wide tract of country to a distant locality, while all the intervening district remained for the time totally unaffected. In the fortress of Iletsk, in the government of Orenburg, the first cases of cholera, which were observed on the 2nd of October, occurred in a soldier and a woman, who were taken ill of the disease while returning together from the city of Orenburg, (forty-two miles distant,) in which the cholera was at that time raging. The intermediate line of country between Orenburg and Iletsk was still unaffected. The soldier and woman, before leaving Orenburg, had been in company with a man affected with cholera, and they both died of the disease on the day after their arrival at Iletsk. The malady soon spread to other persons in the garrison, and Dr. Schimanski, the Staff-Physician, traced out very distinctly the progress of the disease, throughout the first eight cases. The two first victims, the woman and soldier, contracted, as I have just stated, the disease in Orenburg. The husband of the woman was seized three days after her; about the same time two girls, who lived in the immediate neighbourhood of her male travelling companion and fellow victim, and who had visited him soon after his arrival at Iletsk, were attacked. The aunt of these two girls, who nursed them, next suffered; and from her it spread to her two sons. The disease subsequently became diffused with such rapidity, that in the course of twenty days 113 persons had been attacked.”—[Edinburgh Medical Journal, l. c. p. 49.]

“In the official medical report upon the cholera, in the government of Orenburgh, which mentions the above case of Iletsk, it is stated, that in the same province alone, *eight* similar instances had been ascertained judicially, and by physicians who were attending the course of the epidemic, in all of which a person, who had contracted a tendency to the disease, in a place where it prevailed, arrived in an uninfected locality, and

there sickened, and communicated a morbid condition to the atmosphere of his new residence.”

“*St. Petersburg.*—When the cholera invaded St. Petersburg in 1831, the city prison, containing about 400 inmates, remained perfectly free of the disease until the 23rd of June, O. S. when a prisoner who had been sent out some weeks before to a public hospital to be treated for a syphilitic complaint, was returned back to the jail *with a diarrhoea upon her*. She saw and embraced her husband for a moment as she passed to the room of observation. In a few hours her symptoms became those of true cholera, and she died that night, forming the first case of the disease in the prison. The next persons attacked in the prison were three women placed in the same room with the first, and one of whom had rubbed the deceased. These three all died within three days after the first. The husband of No. 1 became next affected. He lived in a separate part of the jail. After this man, others in his room took the disease; and it extended itself ultimately to 27 of the inmates of the prison, in all, 15 of whom died; and Drs. Russell and Barry, who have reported the case on the authority of Dr. Bish, the resident physician, state that there was but one out of these 27, to whom contagion could not at the time be traced. None of the noble class who were lodged in a separate part of the building were attacked.—See *Edinburgh Journal* for February, 1832, p. 175.”

The war in Poland accelerated the invasion of cholera into that unhappy country, into which the Russian army commenced its march on the 5th of February, 1831, in three columns, of which many battalions came from infected provinces. Thus the governments of Volhynia, Grodno, and Wilna, were extensively under the influence of disease in the spring of 1831; during this campaign the Russian army lost great numbers by cholera, and Marshal Diebitch himself died at Pultusk, on the 10th of June, 1831, of a few hours' illness, a circumstance which gave rise to the unfounded rumour that he was poisoned; the details of his illness have been published by an eye-witness, Dr.

Koch, of the Prussian service. In Warsaw the disease appeared on the 14th of April, after the battle of Iganie, where the Poles took many prisoners, who were brought to Warsaw. In Poland the disease advanced and retreated with the infected armies in a striking and remarkable manner. Westwards and southwards from Warsaw, it spread rather slowly towards the Prussian confines, arriving on the 23rd July at Kozięglow, a little town nine miles south of Czenstochowa, and but two German miles from the frontier of Silesia.

Northwards the disease had spread in March and April through Lithuania, to the seaports of the Baltic, particularly Riga. From Riga the cholera advanced through Courland and and Liefland (Livonia).

Petersburgh was now threatened on every side, for the disease broke out with renewed violence in the European provinces formerly affected, while most of those which had hitherto escaped suffered in their turn. Under these circumstances the metropolis, considering the great quantity of goods and passengers who arrive by water-carriage from the interior of the country, could not be expected to remain long exempted, although all possible precautions, short of entirely preventing communication with the country, were adopted; accordingly cholera appeared in Petersburgh in July, 1831. Very serious disturbances arose in the Russian metropolis among the lower orders, who considered the pestilence as artificially produced for their destruction by secret friends of struggling Poland. These troubles were only appeased by the presence of the emperor, but not before the mob had destroyed the cholera hospital, and murdered one of the physicians. During this epidemic seventeen medical men died in Petersburgh, and a great many others were attacked, some slightly, some severely. The hospital nurses, porters, and attendants, suffered in a very large proportion, as did a great number of the mob engaged in sacking the cholera hospitals. Cholera had already invaded several of the most northern provinces of Russia, and had arrived at

Archangel in May, 1831. Archangel is the most northern emporium of commerce in the world, and is the highest latitude attained to by cholera, which in a population of 19,000 destroyed more than 1200. In the beginning of August cholera arrived at Helsingfor; and of September, at Abo in Finland. After this Aland and the neighbouring islands were affected, and so it passed into Sweden. Danzig, 30th May, 1831; Elbing, 11th July; consequently eleven weeks after its appearance in Danzig: but there was an interruption, or rather a great diminution of the intercourse between these towns. From Danzig the disease radiated in every direction throughout the neighbouring provinces. Thorn, 21st July, 1831; Konitz, 22nd August; Memel, 27th July; Königsburgh, 22nd July. Here a formidable cholera insurrection took place. Stettin, 25th August, 1831; Berlin, 30th August; Frankfort on Oder, end of September; Magdeburgh, 3rd October.

From Magdeburgh the disease spread extensively upwards, along the course of the Elbe. Halle, 20th December, 1831; Merseburg, 1st January, 1832; Breslau, 23rd September, 1831. In the first months of 1832, cholera had nearly disappeared from the German provinces of Prussia. Deaths 31,000. Hamburg, 7th October, 1831. Mecklenburg-Schwerin took most extraordinary precautions, and escaped.

Saxony, though Prussia and Austria on either side of it were severely visited, adopted strict measures of precaution, and escaped; the cholera was neither at Leipzig or Dresden! Hanover also escaped, with the exception of Lüneburg, 22nd October, 1831. Sachsen-Weimar, Gotha, Anhalt, Hessia, Brunswick, and some other small principalities, all escaped, and apparently by the same means, viz. non-intercourse with infected places.

In some Saxon villages, as Cosing and Edderitz, the disease broke out but did not spread, apparently in consequence of the measures of precaution instantly put in force by the authorities.

Austria suffered most severely; Brody, (Gallicia,) 5th May,

1831 ; Limberg, 22nd May ; all over Gallicia in 1831. Died 97,770.

Cracow seems to have been infected, not from Poland, but from Gallicia.

Beginning of July, 1831, cholera began in Hungary. In beginning of June, 1831, much popular violence. Spread very rapidly. Pesth, middle of July ; Presburg, 9th September, 1831.

In Hungary cholera had ceased as an epidemic by the beginning of April, 1832, having proved fatal to at least 240,000 persons ! Vienna, 15th August, 1831 ; Prague, 28th November, 1831.

Bohemia was widely affected ; but the disease did not spread from Vienna far either to the south or west, and accordingly Carinthia, Stiermark, and the Tyrol escaped, all being protected by the strictest precautionary measures.

It is worthy of being noted, that cholera remained, as it were, stationary and in a suppressed form during the winter of 1831 and 32, in Hungary, Bohemia, and Germany. It did not spread into Saxony, Mecklenburg, Bavaria, and scarcely into Hanover, although these bordered on infected States, an immunity not to be accounted for by the existence of any natural boundaries, as mountains or rivers, for the limits are mostly conventional between the infected principalities and those which escaped ; many have therefore attributed their escape to the precautionary measures taken. It is strange that Leipsig was spared, while Halle suffered so long and so severely ; the situation of the former city appearing to be much more favourable to the development of *miasma* than that of the latter.

Moldavia, spring, 1831. In Jassy the deaths exceeded 6000, out of a population of 27,000. The disease began in June ; and no doubt its diffusion was favoured by the unhealthy position of the town, and the condensation of a wretched population, chiefly Jews and Gipsies, in its filthy narrow streets. All the medical men, except three, perished with most of their families.

Bucharest, July, 1831 ; Bulgaria, July, 1831 ; Constantinople, July, 1831 ; Adrianople, Gallipoli, Philippopoli, Sept. 1831.

It is to be noted that plague broke out in Constantinople at the same time with cholera ; but while the latter epidemic ceased towards the end of September, the former continued for several months longer. Cholera now a second time invaded Asia Minor, and simultaneously with plague caused great devastations. Corfu, October, 1831 ; Monastori, in Greece, November, 1831.

The destruction of religious pilgrims at Mecca was appalling. The place resembled a field of battle, so great were the numbers of the unburied dead ; and at last even the fanaticism of Musselmen was forced to yield, and the survivors sought safety in a hasty and tumultuous flight. Three-fourths of the pilgrims are calculated to have perished during the three days they were densely crowded together at Mecca ; and of the fugitives 10,000 fell victims on their journey. The Pasha of Egypt now repeated the precautions so successful in 1823, but this time they were taken in vain, because, as is supposed by many, they were not resorted to sufficiently soon ; be this as it may, cholera broke out first at the two quarantine stations, where the pilgrims from Arabia were detained ; and in the middle of August, 1831, it appeared in Cairo and Damietta, and towards the end of the month in Alexandria. Egypt lost on the whole 150,000. The cholera ascended the Nile, and was at Luxor, the site of ancient Thebes, by the end of September.

We next find the cholera visiting England, it arriving about the 4th of November at Sunderland, a seaport directly opposite to, and commercially connected with Hamburgh. The cholera spread to many towns in the north of England, but did not any where rage with very destructive violence, a circumstance attributable perhaps to the more complete separation of families in Great Britain, as compared with our continental neighbours. The existence of the disease was announced on the 27th of January, 1832, in Edinburgh, and on the 10th of Fe-

bruary in London.* The ravages of the cholera in the metropolis were comparatively insignificant, its victims during the whole epidemic not exceeding 1500.

The following very interesting facts connected with the progress of cholera in Scotland, are taken from a highly important *brochure* by my friend Dr. James Y. Simpson of Edinburgh, on the Evidence of the occasional contagious Nature of Cholera.

“ *Edinburgh.*—Before cholera reached Edinburgh, it raged for some time previously in a severe degree in the district of country lying to the east of the city, as in Haddington, Tranent, Prestonpans, &c., and particularly in the town of Musselburgh, six miles distant. The first cases of the disease which were observed in Edinburgh occurred towards the latter end of January, 1832, and were all in the persons of individuals who had been visiting some of the places to the eastward where the cholera was prevailing, and who had consequently been directly exposed to the morbid cause or causes of the malady (whatever we allow these to be) which were operating in these infected localities. The second case (27th January) afforded an instructive example of the great difficulty which is often experienced in endeavouring to arrive at the truth in such investigations as the present. The subject of the case, an Irishwoman, residing in a close off the West Bow, was taken to one of the cholera hospitals, and was for some time conceived to afford the strongest possible evidence against the doctrine of contagion, for she stoutly denied having been out of Edinburgh. During the period of her convalescence, however, she voluntarily mentioned to Dr. Ransford, then clerk to the hospital, that she had been some days previously singing in the streets of Haddington, Tranent, and Musselburgh, and had slept in Prestonpans in the bed of a cholera patient; and she stated that she had been before de-

* The progress of cholera in Great Britain will be found accurately traced in the annexed map.

tered from making this confession, under the dread that she would be punished for bringing the disease into the town.

“None of the three first cases of importation of cholera into the city proved effectual in propagating it to any of the resident inhabitants; and no instance of a person being attacked with the malady, who had not been in the infected eastern districts, occurred until Saturday, the 28th of January, when a woman, widow Mac Millan, died of it in Skinner’s Close, High-street, after nursing her grandson, who was previously ill of the disease, and had been exposed to its contagion by residing in a house in Musselburgh, in which several fatal cases took place. Professor Alison has been so kind as to draw up for me the history of these two cases, in as far as they bear upon the question of imported contagion; and I shall here give the communication with which he has favoured me in his own words, and with his own excellent prefatory remarks and comments.

“‘It seems to me clear,’ he observes, ‘that the evidence of the contagious nature of any disease turns ultimately on a calculation of chances. The question always comes to this,—Is the circumstance of intercourse with the sick followed by the appearance of the disease, in a proportion of cases so much greater than any other circumstance common to any portion of the inhabitants of the place under observation, as to make it inconceivable that the succession of cases occurring in persons having that intercourse should have been the result of chance. If so, the inference is unavoidable, that that intercourse must have acted as a cause of the disease. All observations which do not bear strictly on that point are irrelevant, and in the case of an epidemic *first* appearing in a town or district, a succession of two cases is sometimes sufficient to furnish evidence, which, on the principle I have stated, is nearly irresistible.

“‘For example, in the case of Widow Mac Millan, in Skinner’s Close, it is certain, as the whole town was under medical surveillance at the time, and every one on the watch for cases of even suspicious cholera, that she was the *first* person in

Edinburgh or Leith (i. e. in about 160,000 people) who took the disease without having been in the district of Musselburgh, Tranent, &c., where it prevailed; nor was there any case in Edinburgh or Leith in a person who had not left the town for ten days after. And in regard to this first case of the disease in Mrs. Mac Millan originating in Edinburgh, the following points were ascertained by a judicial inquiry or precognition made, at the request of the Board of Health, by the Sheriff of the county, who examined different witnesses on each point till he was perfectly satisfied of its truth. 1st, That the woman herself had never been out of the Close in which she lived, during the existence of the disease in the neighbourhood. 2nd, That her son, a hawker, had slept in a house in Musselburgh in which a woman was dying of the cholera, on the Monday. 3rd, That after returning to town, he was seized on Wednesday with vomiting and purging of whitish or watery matter, cramps, and feeble pulse. I saw this lad myself on that day, and immediately suspected that he had been at Musselburgh, which was at the time denied, but afterwards admitted, and confirmed by abundant other evidence. 4th, That Mrs. Mac Millan was with him during the day, in a small confined room, rubbing his limbs and nursing him, and he recovered under the use of opiates and stimulants. 5th, That on the Saturday, when he was convalescent, she was seized with the disease in its most virulent and unequivocal form, and died in ten hours. Now I presume it will not be denied that the epidemic cholera, which was never known in Edinburgh before 1832, and has not been seen in it since 1833, must have some cause or causes of local and temporary existence only. That the lad Mac Millan, who had slept a night at Musselburgh, (then much affected with the disease,) should be seized with it, proves nothing as to the question, whether intercourse with the sick has the power of exciting the disease or not. But if that intercourse has no such power, it is plain that his mother, who never left her own Close, had no more business to take the disease than any other of the inhabi-

tants of Edinburgh or Leith, and her infection must have been a mere chance. The chances therefore, are, nearly 160,000 to 1 against her being the first person in Edinburgh or Leith who should take the disease, and almost infinite to one against her being infected by it within sixty hours after her son.

“‘From the time, therefore,’ Dr. Alison adds, ‘when I was satisfied as to these facts, I have never doubted of the disease having a contagious property, although I have never thought it proved that its extension is to be ascribed to that property alone.’”

“In many of the more isolated cases which occurred during the prevalence of cholera in Edinburgh, the evidence of previous exposure to the infected could not be distinctly ascertained.—(See on this subject Dr. Craige’s able Monograph upon the disease in this city, in the *Edinburgh Medical Journal*, vol. xxxix. p. 366, &c.)

“In each of the three instances we have next to bring forward, the city of Glasgow formed the infected focus from which the infection was carried.”

“*Campbelton*.—In this town, which contains between 9000 and 10,000 inhabitants, and is situated on the west coast of Argyleshire, there were two separate irruptions of cholera in 1832; the first in April, and the second towards the latter end of July. During the first irruption only thirteen cases occurred, ten of which were fatal; and through the kindness of Dr. Macdonald, of Ballyshear, I have been favoured with some notes of them drawn up by Dr. Macintyre of Greenock, who was, at the time mentioned, surgeon to the Cholera Hospital at Campbelton. The first case is remarkable on account of the latent stage, and of the premonitory diarrhoea. 1. Archibald Witers, aged 28, went from Campbelton to Greenock and Glasgow early in April. He himself confessed, (as I am informed by Dr. Macdonald, and the same, Dr. Macintyre states, was admitted by Witers’ mother,) that while in Glasgow he slept in a house where cholera existed. In returning home, he came by a steam-

boat to Tarbert, and travelled from thence to Campbelton by land, (a distance of thirty-seven miles,) without entering any house on the road. He reached town on the 10th April. On the 15th he was attacked with diarrhœa, and by the morning of the 17th had also some nausea and vomiting. Dr. Macintyre visited him at 4 o'clock, P. M., on the latter of these days, (the 17th,) and found him affected with all the characteristic symptoms of malignant cholera. On the 19th, however, he was convalescent, and had no consecutive fever. 2. Mary Morrison, aged 7, who had been in Witors' house when he was ill, was seized at 7 P. M., on the 20th, with decided cholera, and died in twenty-two hours. 3. Mrs. Morrison, the mother of No. 2, was attacked with the disease at 6 A. M., on the 21st, and sunk in about twenty-one hours. 4. Mrs. Witors, the mother of No. 1, who had attended and nursed her son during his illness, sickened in the afternoon of the 21st, of cholera, and died on the 22nd. This last patient died in the Cholera Hospital. 5. Alexander Macneil, aged 70, residing in a street about 600 yards distant from that in which Witors and Morrison's houses were situated, was attacked with cholera symptoms on the forenoon of the 22nd April. 'He had,' Dr. Macintyre remarks, 'been in conversation with one of the hospital attendants the night previous, and had been working, I believe, with Witors on the 15th and 16th, when the latter was suffering under diarrhœa.' Macneil died on the morning of the 23rd. 6. His wife, aged 70, was seized with the disease about 12 o'clock noon, on the 25th April, and died after an illness of thirteen hours. 7. A girl, Sinclair, sister-in-law to one of the porters at the hospital, was taken ill on the 23rd, but ultimately recovered. 8. Mrs. Christie, aged 38, sickened on the morning of the 28th, and died on the 29th. 9. Her husband, Malcolm Christie, was attacked at 5 A. M., on the 29th, and died on the 30th. 10. Robert Wishart, residing in a house adjoining to that of Christie, was seized at 4 A. M., on the 30th, and died at 8 P. M., of that day. 11. Mrs. Wishart, aged 35, wife of No. 10, was

attacked the same day, at 5 A. M., and died next morning. 12. Donald M'Killop took the disease on May 1st, and died on the succeeding day. 13. Fawcett, aged 4, living in the same house with the Wisharts, (Nos. 10 and 11,) was attacked on the 3rd, but recovered. Dr. Macintyre does not state in the communication which has been put into my hands, whether or not any intercourse, direct or indirect, was traced between the Christies, Wisharts, and M'Killop, and those previously affected.

“ The thirteen cases that have been mentioned, all occurred within the space of eighteen or twenty days. After the last of them, (Fawcett's,) on the 3rd May, no other cases of cholera were seen in Campbelton till the 28th July, when the second irruption of the disease occurred, and between that date and the 28th October, 98 of the inhabitants were attacked with cholera. No importation of the disease in this second irruption could, as has happened often in regard to the subsequent returns or recurrences of the disease in other places, be distinctly ascertained.

“ *Greenock.*—Mr. Turner, surgeon, Greenock, has kindly favoured me with the following facts relative to the first introduction of cholera into that town.

“ 1. Dow, an elderly fishmonger, belonging to Greenock, went to Glasgow in the latter end of February 1832, and slept in that city in a tenement in which cholera was at that time raging. Next day he came to Greenock labouring under a diarrhoea, which, after continuing for two days, terminated in all the more marked and fatal symptoms of confirmed cholera, and the man speedily sunk. 2. Mrs. Black, who officiated as nurse to Dow, took the disease next day, and also died. 3. On the following day Dow's wife was attacked, but recovered.

“ 4. A fisher boy, the name of M'Millan, who was some hours in company with Dow, and drinking with him on the day he came from Glasgow, and when he was labouring under the premonitory diarrhoea, was seized with cholera that night, and

died on the following one in the hospital. 5. Next day this boy's mother took the disease, and died in a few hours. 6 and 7. On the following day his father and brother had also a severe attack of cholera, but their cases did not prove fatal. The disease subsequently spread through Greenock to a considerable extent. The boy M'Millan had been out with his boat in the river, fishing, but with this exception, and that of Dow, who had been in the infected house in Glasgow, none of the other persons who have been mentioned had been for a considerable period out of Greenock, and the disease had not been previously seen in that town. In other words, Dow's nurse and mother, M'Millan, his mother, father, and brother, had only had intercourse with a previously infected locality, *indirectly* through the person or body of Dow, who had been, as already stated, exposed to the disease in his lodgings in Glasgow.

“*Doura*.—At the small village of Doura, in Ayrshire, containing 37 families, and 170 inhabitants, not less than 21 cases of confirmed cholera appeared. The first case which occurred was in a young woman who had, on the 20th February, 1832, travelled on foot with her husband from Springbank, near Glasgow, where the disease was then raging, to Doura, a distance of about twenty or twenty-two miles. She was intending to proceed onwards to Kilwinning, but she was seized with purging and vomiting on the road, and was so ill and exhausted by the time she reached Doura, that she could proceed no farther, and took up her abode in the village, in the house of an acquaintance. She expired the following evening, after having exhibited all the best marked symptoms of cholera, as recognized by the surgeon in attendance, who had seen the disease in India. The stranger was attended by two female villagers, (sisters,) who rubbed her, &c. One of these sisters was attacked with cholera on the 24th, and died after twelve hours' illness. On the 25th, the other took the disease, but recovered. No restrictions whatever, as to intercourse with the infected persons were put in

force, and so rapidly did the disease spread, that by the 9th of March, 21 cases in all had occurred among the above-mentioned small population.

“ In this instance, as in several others which we have mentioned, a person travelling from an infected district, and actually labouring under symptoms of cholera, appeared to carry the disease through a line of healthy country to a distant locality. No case of cholera existed at the time anywhere near to Doura, and immediately previous to the arrival of this woman, the inhabitants were in the enjoyment in every respect of their usual state of health ; yet, within the space of seventeen days after the infected stranger arrived among them, one out of every five individuals in the place was attacked with confirmed cholera ; six died of it ; and almost every inhabitant was affected in a greater or less degree with diarrhoea. (See Mr. Salmon of Frome’s Letter in the *Lancet* for 1832, p. 182, and also Mr. Moir’s *Proofs of the Contagion of Malignant Cholera*, p. 64.)”

“ *Carnwath*.—In the parish of Carnwath, Lanarkshire, containing a population of about 5000 individuals, five cases of pestilential cholera were observed during the prevalence of the disease in this country in 1832-33, and the first of them was imported. Dr. Wilson, who was chairman of the Local Board of Health, has favoured me with the following particulars relative to the history of these five cases.

“ The first instance of the disease was observed in the person of a stranger of the name of Waters. This man was a native of Beith in Ayrshire. He was travelling homewards to his native place, asking charity, after having a few days previously left off his employment as a mason in the more eastern districts of the country, where the cholera was raging. It was not ascertained from him at the time whether he had been directly exposed in the east to the contagion of cholera, but he was taken ill with the premonitory symptoms of the disease on the road, at some distance from Carnwath, and when he reached Braehead, (a village in that parish containing about 150 inhabitants,) on

the —th of June, 1832, he was *already* labouring under a bowel complaint, and unable to proceed farther. He took up his quarters in Braehead, in a lodging house kept by a person of the name of Telford, and all his symptoms having speedily become more severe and characteristic of cholera, he was sent onwards in a cart by the alarmed villagers, into the adjoining parish of Carstairs. He died either that night or on the following morning.

“ The day on which Waters was removed from the lodging-house in Braehead, one of Telford’s (the lodging-house keeper) daughters, and Jean Gibson, cleaned the clothes of the house, and were both taken ill that evening. Gibson died within thirty hours with the most distinct symptoms of cholera ; but the girl Telford recovered after a severe attack. At this time two hawkers (a man and his wife) came to Telford’s. They both sickened with cholera, the woman recovered, but the man died after a few hours’ illness.

“ The local Board of Health adopted every possible measure to prevent the spread of the disease. No other cases of cholera previously or subsequently occurred in Carnwath, or any where within a circuit of country of seven miles extent, at least, around it.

“ *Glen*.—This is a healthy little village, situated about two miles south of Falkirk, Stirlingshire ; and inhabited by between thirty and forty families of colliers. In April, 1832, eight cases of cholera occurred in it, the two first of which were in individuals who had been exposed to the contagion of the disease in another distant locality. My friend, Mr. Graham of Polmont, who has the villagers under his professional charge, has furnished me with some interesting particulars regarding the history of the disease among them.

“ 1. and 2. Robert Anderson and his wife, after spending some nights at Airdrie, (about fourteen or fifteen miles distant,) in a house where some of their relations had died of cholera, returned from thence to Glen on Wednesday, the 4th April. At

the time of their arrival were both labouring under the premonitory diarrhœa, and, as was afterwards confessed, they were themselves convinced that they had taken cholera. They kept their complaints, however, as secret as possible, in consequence of the strong feelings of prejudice then existing regarding it, until Sunday the 8th, when Mr. Graham was sent for by the overseer of the mines at the village. The woman was by that time collapsed, and died next night. Anderson himself had the characteristic diarrhœa to a great extent; but, being of a strong constitution, he ultimately rallied after lying in a state of collapse for thirty-three hours. 3 and 4. Two of Anderson's children, who had remained at home while their parents were visiting the infected house of their relatives in Airdrie, were seized with distinct, and, in one of them, severe symptoms of cholera on the 12th. They both recovered. 5. A man, named Benny, was suddenly taken ill with all the symptoms of cholera on the morning of the 10th. He was immediately subjected to active medical treatment, and recovered. Jenckens, another man, was similarly attacked on the evening of the same day, and also did well. Mr. Graham does not state what particular communication the two last persons, Benny and Jenckens, had with the Andersons. 7. A daughter of Jenckens, (No. 6,) was attacked on the 11th; and the eighth and last case occurred on the 15th, in the person of Alexander Brown, who had gone repeatedly to look at Anderson's wife while she was dying.

“Ferryden and Boddin.—The following facts with regard to the introduction of cholera into Ferryden and Boddin, two villages in Angus-shire, Scotland, have been drawn up by Dr. Brewster, the brother of the distinguished Sir David Brewster, and clergyman of the parish in which Ferryden and Boddin are situated. I am indebted for the communication of them to Professor Alison.

“The village of Ferryden is placed on the south bank of the South Esk, opposite to the town of Montrose, and contains about 700 inhabitants. The district of country in which it lies,

remained altogether free of cholera, when in 1832, and the earlier months of 1833, the disease was prevailing in different parts of the kingdom. In the end of June, 1833, the smack *Eagle*, from London, arrived at Montrose. Two cases of cholera had occurred amongst the crew during the passage from London, one soon after the smack left that port, and the other off Harwich. As soon as the vessel reached Montrose river, the crew dispersed to their several homes. One of them, Robert Findlay, an inhabitant of Ferryden, carried his clothes and bedding to his house there. A day or two afterwards, two children in the village, who were reported to have been seen tumbling during the preceding day on Findlay's mattress as it was laid out to the air, were seized with rapidly fatal cholera, and died on the 2nd of July; and this, it may be proper to remark, took place at a time when the disease was considered to have nearly or entirely disappeared from Scotland. On visiting Ferryden that day, Dr. Brewster found the mother of the two children labouring under a fatal attack of cholera. The malady subsequently spread through the village, but not rapidly; and during the four weeks it continued, it carried off 27 out of the 700 inhabitants, or nearly 1 out of every 27 of the residents. It appeared (Dr. Brewster observes in the communication with which I have been favoured) in different parts of the village in succession, and almost uniformly among the relatives, visitors, and neighbours of those who were previously affected. Out of the few cases, he adds, which appeared in Montrose, two were relatives of the sick in Ferryden, whom they had gone to visit there, and were themselves seized with the disease after their return home. The inhabitants of the adjoining district of country in general carefully avoided all communication with Ferryden, and the disease only appeared in one other part of the parish, viz. in Boddin, a small village on the sea coast, nearly three miles south from Ferryden. Only two cases occurred in this locality, but these two afforded strong corroborative testimony of the contagious property of cholera. Margaret Stott, a young woman, an inha-

bitant of Boddin, went to visit her sister at Ferryden, and upon returning to Boddin, was seized with the disease, and died in two days. Jean Peterkin, an aged woman, who lived in the house adjoining to that of Stott, and who had not been out of the village, assisted, amongst other things, in putting Stott's body into the coffin, and afterwards washed her bedding. In the course of two or three days she had a fatal attack of cholera. No other person (Dr. Brewster adds) in the village of Boddin, or in the parish, or in the surrounding district, with the exceptions now noticed, was affected by the disease."

In addition to the above important facts detailed by Dr. Simpson in his *brochure*, he has furnished me with the following Table, compiled carefully from authentic information, and exhibiting the dates of the arrival of cholera in many places in Scotland. He adds, on the authority of Professor Traill, that cholera never reached the Orkney or Shetland Islands, although it was as far north as Thurso.

A LIST

Of several Towns and Villages in different Parts of Scotland which were visited with Asiatic Cholera, showing the Date of its Appearance in each particular Locality.

TOWNS.	DATE OF APPEARANCE.
Haddington,	17th December, 1831.
Edinburgh,	See Dr. Craigie's Paper in the Edin. Med. Jour. vol. xlix.
Tranent,	
Musselburgh,	12th January, 1832.
Leith,	19th January, 1832.
Kirkintulloch, seven miles from } Glasgow,	26th January, 1832.
Glasgow,	21st January, 1832.
Paisley,	12th February, 1832.
Greenock,	13th February, 1832.
Falkirk,	26th February, 1832.
Doura, near Kilwinning, Ayr- } shire,	About 3rd March, 1832.
Perth,	6th March, 1832.
	13th or 14th March, 1832.

TOWNS.	DATE OF APPEARANCE.
Inverary,	21st May, 1832.
Water of Leith, near Edinburgh,	12th March, 1832.
Rothsay,	23rd March, 1832.
Larbert near Stirling,	Before 1st April, 1832.
Fort George,	7th May, 1832.
Dundee,	23rd April, 1832.
Stirling,	10th July, 1832.
Helensdale, on the confines of } Caithness and Sutherland, . }	About 23rd July, 1832.
Portpatrick,*	7th August, 1832.
Ayr,*	About 15th August, 1832.
Aberdeen,	27th August, 1832.
Cupar, in Fife,	30th August, 1832.
Tain,	8th September, 1832.
Inverness,	24th August, 1832.
Dumfries,	15th September, 1832.
Fort William,	24th September, 1832.
Crieff,	2nd October, 1832.
Hawick,	About 20th October, 1832.
Island of Islay,	About 23rd October, 1832.
Kelso,	About 29th October, 1832.
Nairn,*	About 11th August, 1832.
Wick,*	21st July, 1832.

It is exceedingly remarkable how many of the great towns of England either escaped infection altogether, or were visited by only a trifling outbreak of the disease.† Up to the 24th of June, 1832, (that is during a period of about eight months since its first appearance at Sunderland,) the total number of cases throughout Great Britain, inclusive of London, amounted to only 14796, and the deaths to 5432.‡ The disease, it is true, continued in many places to linger long after the above date, and reappeared as an epidemic in some places in 1833 and 1834, but still we are quite warranted in concluding, that on the whole in Great Britain and Ireland, the cholera did not count 30,000

* "See some additional dates in my Paper on Cholera."—Dr. Simpson.

† Cholera commenced in Liverpool on the 12th May, and in the mean time had visited Hull, York, Leeds, Manchester, and Warrington.

‡ Medical Gazette, vol. x. p. 400.

victims. In Ireland, particularly in Dublin and Sligo, the mortality was much greater than in England, an occurrence which may, perhaps, be accounted for by the bad diet of the Irish lower classes, and the crowded state of their dwellings, it being well known that in the worst quarters of the city, many families reside on the same floor, and frequently more than one in the same room. "In London," says Dr. Elliotson,* "the greater part of the people are well fed, better fed than in any other part of the world; they eat more meat, and that flesh is of such quality as scarcely to be found in any other country; besides which, they are better clothed and more comfortable, and instead of trashy wines they have good sound ale and porter, and malt liquor of all kinds. But in Paris the water the inhabitants drink is very bad; the people are crowded together, I know not how many families in a house, with little ventilation; the streets are narrow, the houses dirty, and the population live upon what Englishmen consider trash, not roast-beef and mutton, but all sorts of dishes made up of bread and vegetables, with a little meat boiled in water to colour it or give it a flavour; and drink not good beer but thin wine."†

Certain it is, no matter how we may attempt to account for it, that cholera was much more destructive in Paris than in London, 385 deaths having occurred in one day, 8th April, 1832, in the former city. Nothing has puzzled and perplexed the continental physicians more than the comparative immunity from cholera enjoyed by England, notwithstanding their predictions, that *there* its ravages would attain to a *maximum*, for they contended, that in the English towns many circumstances would contribute to render the disease more liable to spread, as for example, their very dense population, the extreme poverty and bad diet of the lower orders, and the damp, foggy nature of the climate. Now, I believe, that the reproaches made by foreigners respecting the extreme penury of the lower orders in England

* Medical Gazette, vol. xii. p. 628.

† At the end of this Paper will be found a list of *all the towns* in Great Britain where cholera occurred.

are not well founded, at least comparatively speaking, and with reference to the same class of persons in the continental cities; and I am persuaded that in English cities the diet of the poor is superior to that of the continental poor. Indeed foreign physicians have tried their ingenuity to account for the slowness of the ravages of cholera in Great Britain, some attributing the immunity to tea, some to the quantity of meat we consume, and some to the vapours arising from our numerous coal fires; and each of these hypotheses has been met by objections, for the Chinese, the most national tea drinkers in the world, were wofully scourged by cholera; and the city of Halle, in Germany, the most devastated town of that kingdom, uses nothing but coal for firing. It is to the more substantial nature of English fare, to the superior cleanliness of that nation, and to their living in families separated from each other, that we must attribute their comparative exemption from cholera, an exemption the more remarkable, when we consider that in England, commercial and private travelling between town and town is more rapid, and ten times more frequent than on the continent.

Cholera first appeared in Paris on the 24th of March, 1832, and it has been argued by those who deny the contagious nature of cholera, and its importation from abroad, that in France it broke out suddenly, not on the confines, but in the heart of the kingdom, and consequently that it must have arisen spontaneously in the metropolis. Before we attach much weight to this argument, we must have very strong proofs that the facts are as above stated; now it is very remarkable that cholera was officially announced to exist at Calais only eight days after it appeared at Paris, and when we recollect how unwilling the authorities in all seaports of hitherto uninfected nations, have invariably been to acknowledge the existence of cholera, it is not by any means improbable that cholera may have existed in Calais before it broke out in Paris, a supposition confirmed by the report of Arnaud, Moribaud, and Gendrin, who witnessed in Calais, towards the end of 1831, many very violent cases of cholera greatly resembling the Asiatic; nay, even after the cho-

lera had manifestly appeared in Calais, many persisted in declaring that its victims died of common enteritis.

Indeed, according to Moreau de Jonnes, (*Lancet*, 1832-33, p. 689,) cholera appeared at Calais on the 15th of March, and at Paris on the 24th; so that France cannot be considered as forming an exception to the general rule, that the *disease always appears in seaport or frontier towns, before it makes its way to the centre of any country*. Once arrived at Paris it spread in every direction. The following observations are from Moreau de Jonnes: "The disease spread by contiguity, as in the other parts of Europe, following lines, of which Paris was the centre, ramifying with the communications through the country. In each department the time of the irruption was sooner or later after that of Paris, in proportion to the distance from, and the frequency and rapidity of the communications with the capital. Thus the following departments of the east became infected in the following dates, viz.—

Seine,	March 24, 1832.
Seine and Marne, April	2, „
Marne, „	16, „
Meuse, „	11, „
Moselle, „	27, „
Meurthe, . . . May	4, „
Vosgey, „	13, „
Hante Saonne, . June	16, „

“ In the western departments the disease broke out as follows :—

Seine,	March 24.
Seine and Oise, . „	28.
Eure and Loire, . April	8.
Indre and Loire, . „	19.
Deux Serres, . . „	25.
Vendée, July	10.
Charente Inferieure, August	4.
Charente, „	30.

“ The degree of rapidity with which the disease spread in different directions is as follows. Cholera appeared in Calais on the 15th of March, 1832, and broke out at Arles on the 17th of September following, having thus, in 186 days, traversed 200 leagues, forming the great diameter of France from north to south. The disease was recognized in Paris, the centre of the kingdom, on the 24th of March ; on the 27th of April following it had spread by contiguity to the department of the Moselle, and on the 11th of May, to that of Finisterre, taking thirty-five days to reach the eastern, and fifty days to arrive at the western frontier of France ; having traversed on the one side seventy leagues, and on the other one hundred and twenty.

“ Thus the cholera traversed France, from north to south, at the rate of one league in twenty-four hours ; whilst from east to west, it required but eighty-five days to travel a distance of 190 leagues, which gives a rapidity greater by one-half.”

These statements of Moreau de Jonnes, are of the greatest importance. We see cholera introduced probably from England to Calais, and immediately after to Paris, from which it radiated in all directions by slow and varying stages all over the kingdom. The position of Paris, and its daily communication with England, rendered it almost the first prey of the disease in France. Once there, the cholera moved along the different lines of communication in every direction, its route not governed by any of the laws observed by epidemics depending on atmospheric changes ; and its gradual progress from Paris, as a centre, towards all parts of the circumference of France, presenting a course obviously opposed to that of such epidemics. The following quotation relative to cholera in Paris is taken from Dr. Simpson's *brochure*.

“ *Paris*.—Mr. Velpau, in an essay on the cholera in Paris, contained in the twenty-ninth volume of the Archives Générales de Medecine, mentions several examples of the contagious propagation of the disease in that city ; and we can only consider his testimony to the doctrine of contagion as the more im-

partial and valuable, seeing that he was one of those medical men who signed the celebrated official document, on the fourth day of the appearance of the malady in the French metropolis, denying altogether its contagious character. ‘Out of eighty and odd cases, (he remarks, p. 224,) in this city, the history of which I have noted, there is none the subject of which had not some previous communication, direct or indirect, with other cholera patients.’ Among other series of cases he gives the following, (p. 222 :) ‘A man, aged 55, had remained near two of his friends who were affected with cholera. He was attacked himself, three days afterwards, at his own house, (Rue Vieille du Temple,) and died in eight hours. 2. His eldest son, aged 23, who took charge of his father to the last moment, was attacked next morning, and died in seventeen hours. 3. The mother; 4, next the daughter; 5, then another son, who came from his work to the assistance of his sister, were successively attacked with the disease, but in a less severe form. 6. A child of ten years, who came to visit them, was also seized. 7. An artist living in the flat above was next affected, as well as 8, his wife; and 9, another person died of the disease in the same house, which (M. Velpeau adds) was not more insalubrious than the neighbouring habitations. The above persons were all (he likewise states) regular in their habits, and in comfortable circumstances.’

“In the surgical ward, St. Jean, of the Hospital La Pitié, cholera (M. Velpeau again observes, p. 225,) did not appear till after the nurse attached to it had been engaged in attending cholera patients in the medical wards. The first person attacked in the ward passed constantly by the bed of the second. The third occupied the bed nearest his, and the servant of the ward was next seized. A fifth, sixth, seventh, and finally an eighth case occurred among the persons lying in the beds nearest those attacked. 9. A young pupil, who, up to that time, had abstained from visiting cholera patients, came to the hospital in the morning. He was affected with cholera that night at his

own house ; and 10, his brother, who lodged in the same room with him, had a fatal attack of it on the following day."

From England cholera soon spread to Ireland ; the following dates of its arrival were communicated by Dr. Barker, whose official situation in the Board of Health, gave him the best opportunity of ascertaining the progress of the disease.

PLACES.	DATES OF OUTBREAK OF CHOLERA.
Dublin,	22nd March, 1832.
Arklow,	8th April, „
Banbridge,	9th April, „
Cork,	12th April, „
Ramelton, County Donegal, . .	12th April, „
Naas,	13th April, „
Belfast,	14th April, „
Warren's-point,	17th April, „
Stranorlar, County Donegal, . .	22nd April, „
Tralee,	28th April, „
Galway,	12th May, „
Limerick,	14th May, „
Waterford,	1st July, „
Wexford,	21st August, „

It is worthy of remark that Dublin, Cork, and Belfast were affected about four months before Waterford and Wexford. Now a steamer plies twice a week between Dublin and Cork, and Dublin and Belfast, *while there is no direct communication by steam* between Dublin and Waterford, or Dublin and Wexford ; and consequently it appears probable, from the dates, that Cork and Belfast were infected from Dublin, while Waterford and Wexford escaped for many months, not being exposed to infection from this source. At all events, the fact that Waterford and Wexford should have remained so long without the disease is very remarkable, and if not sufficiently accounted for by their more indirect and less frequent intercourse with Dublin, it may perhaps be explained by their trade with England consisting chiefly of the export of agricultural produce, rather than the interchange of passengers.

We have hitherto followed the route of cholera in the old world, we have now to trace it in the new.

“The disease commenced about the 8th of June, 1832, in Quebec, in boarding houses and taverns in the *Cul de Sac*, a low, uncleanly, and ill ventilated part of the city, crowded with emigrants of the lowest description, with sailors, and other persons of irregular habits.”*

Thus we find that cholera appeared in America first at Quebec, just at the season when the spring stream of emigration from England reaches that city. The following account proves that cholera might be thus transmitted.

“The following letter from the surgeon of the British barque Brutus, to the president of the Board of Health of Liverpool,† conveys the melancholy intelligence of the cholera having broke out among the passengers, *eight days after* leaving the river Mersey, and which induced the Captain to put back. It appears from a statement subjoined to the letter, that between the 27th of May, the period when the first person was attacked, and the 13th of June, the day on which the vessel arrived at Liverpool, 117 cases had occurred, 81 died, and 20 had recovered.

‘SIR,

‘With the deepest feelings of regret, I have the painful duty to perform of transmitting to you one of the most melancholy and distressing accounts of cholera, which occurred on board the British barque Brutus, bound for Quebec, from Liverpool, with three hundred and thirty passengers. The first case presented itself on the 25th of May, (being the eighth day after we left the river,) in a strong, healthy man, thirty-five years of age; the symptoms were all well marked, the spasms particularly severe; under the usual means of treatment he recovered. The next case was an old woman of sixty, who died in ten

* Vide the official Report of the Board of Health, Quebec Cholera Gazette, p. 72.

† Cholera Gazette.

hours after the commencement of the attack. The disease continued gradually to increase, (notwithstanding every means having been employed to arrest its progress,) until the night of Saturday, the 2nd of June, when we were a good deal tossed about by a heavy sea, and dark hazy weather; it spread to such an alarming extent, that on Sunday, most of the ship's crew being attacked, and having lost some of them the week before, we were obliged to bear up again for Liverpool. It is impossible to describe the scene of misery on the third, fourth, and fifth, people dying in every direction—the greater number of them destitute of the common articles of bed covering. On the sixth, the weather became more favourable, the disease less severe, and the number of new cases diminished, which has since been on the decline.

‘I have the honour to be, Sir, your obedient humble Servant,
 ‘W. W. THOMPSON, M.R.S.C. in London.’”

“*Brig Amelia*.*—On the 19th October, 1832, the brig *Amelia* left New York (a city in which epidemic cholera at that time prevailed) for New Orleans. The brig had on board her ordinary crew, and one hundred and five passengers, some of whom had the disease *before* sailing; on the sixth day out, the sickness, according to the captain's subsequent official deposition, commenced in the vessel; and in the course of eight days more, or, in other words, by the 31st of October, twenty-four of the individuals on board had died of it, and several more were labouring under the disease. During the course of this last-mentioned day, (31st October,) the vessel was stranded on the beach of Folly Island, a low and sandy island about twenty miles from Charlestown, and far out to seaward.

“The island was the property of a Mr. Milne, who kept four negroes upon it as permanent occupiers, and used it himself as an agreeable summer retreat. Mr. Milne allowed the captain, passengers, and crew of the brig to take refuge in his buildings. The deputy port physician of Charlestown, Dr.

* Dr. Simpson's *brochure*.

Elfe, was sent down to visit the island, and pronounced the disease which he found there to be cholera. A boat's crew of wreckers who had gone down from Charlestown to the island to attempt to save the vessel and cargo, having returned to the city, one of the men belonging to the crew was there seized with well marked symptoms of malignant cholera, and died. The rest of this boat's crew of wreckers were ordered back to the island to perform quarantine, and after having embarked, two fell sick, and one died of cholera on their passage down. Two physicians, Drs. Jewey and Pritchard, were sent to the island to afford the necessary assistance, and being worn out by constant exertions, they were relieved in the course of a week by a third medical officer, Dr. Hunt. There were sent down also from Charlestown a clergyman, the man who had nursed the wrecker that died in the city, and a lieutenant and eighteen men of the city guard, the latter being ordered to the island to perform the duty of a *cordon sanitaire*, and to prevent any individuals from leaving the spot. The wreck of the brig was burned on the 8th November. New cases, however, of cholera continued to occur up to the 17th; and it is important to remark, that some of those who were attacked had never visited the brig.

“In this way there were collected upon Holly Island about one hundred and fifty individuals in all; and amongst these there occurred up to the 17th November twenty-three deaths, or sixteen per cent. of the number fell victims to cholera, including twelve of the passengers landed from the brig, six of the wreckers from Charlestown, (not reckoning the one who died in Charlestown, and the other who died in the passage down,) three of Mr. Milne's four negroes, the nurse, and one of the city guard from Charlestown. Of the other seventeen men belonging to the city guard, and whom it was found impossible to prevent from communicating with the passengers dispersed over the island, every one was affected more or less with the symptoms of cholera, with the exception of the lieutenant; and

nine of them were reported to have been attacked seriously. One of the three physicians, Dr. Hunt, was attacked on the 17th by the disease, but recovered."

"*Smack Trusty*.*—The Leith and London Smack Trusty arrived at London on the 19th of February, and after remaining in port for fourteen days, sailed again on the 4th March, with ten of a crew, including the captain and mate, and six passengers on board. In London the cholera was then prevailing to a considerable extent, more particularly on the side of the river, and in the quarter connected with the shipping. 1. On the morning of the 6th, the cook was attacked with cholera at sea, and died on the 7th. 2. Another seaman complained of headache when the vessel was brought up in Leith roads, (a distance of about 400 miles from London,) on the evening of the 8th. This man assisted on the same night another of the crew and a cabin boy to pull ashore the six passengers in the boat belonging to the smack. When he reached the harbour, however, he found himself so unwell as not to be able to return on board, and died of cholera the next morning in the Leith Hospital at 12 o'clock. This was the second case of the disease observed in Leith, the first having occurred about a month previously in a man who had been visiting his infected relations at Musselburgh. 3. The other sailor who landed with him was also obliged to remain on shore, and had an attack of cholera that night but recovered. 4. On the morning of the 9th, another of the crew of the smack was seized with vomiting and purging immediately

* "The following case of the Trusty has been already imperfectly, and, in several respects, inaccurately stated in Mr. Moir's *Proofs of Contagion*, p. 71; and in the *Cholera Gazette*, p. 264. To ascertain as accurately as possible every particular connected with it, I visited last summer the quarantine station, and had an opportunity of conversing with one of the nurses who was attacked, and with other persons officially employed there during the existence of the disease. I got access also, through the kindness of Dr. Forsyth of Inverkeithing, the quarantine medical officer, to the quarantine official ship journals, in which the state of the sick, the arrival and release of all vessels, the condition of their crews, and all other correlative circumstances, were minutely entered twice a day."—Dr. Simpson's *brochure*.

after the preventive boat visited the smack in the roads; and the vessel having been ordered ten miles up the river to the quarantine station at St. Margaret's Hope, he was, when the vessel arrived there about 5 o'clock of the same evening, transferred to the hospital ship *Nymphe*. The *Nymphe* (one of the old men-of-war employed as quarantine vessels in the station) had been previously cleared out as an hospital ship to receive a suspicious case of cholera which had occurred on board one of the vessels under quarantine, on the 2nd March, but this man had been discharged as well on the 5th. At the time that the sailor from the *Trusty* was placed in the *Nymphe*, on the evening of the 9th March, the *Nymphe* had only two mariners left on board, who had volunteered to act as nurses. On the 11th, two other mariners were joined to these, and on the 16th a fifth was added. The sailor from the *Trusty* had a severe attack, but continued in life till the 16th. 5. In the afternoon of the 10th, a second sailor was transferred from the *Trusty* to the *Nymphe* labouring under cholera: he recovered. 6, 7. On the 11th, two new men were attacked and sent to the *Nymphe*, both of whom died. One of them was a Leith porter, who had come on board the vessel in Leith Roads, having returned with the boats which landed the passengers. He and the pilot (who had joined the smack near the mouth of the river in Dunbar Bay) having been found on board by the officers in the preventive boat, were obliged to proceed with the vessel to the quarantine station. This porter, Murray, was cut off by the disease after an illness of only twelve hours' duration. 8. On the 12th, another of the crew of the *Trusty* was attacked and transferred to the *Nymphe*, but recovered in the course of a few days.

“ Thus out of the whole crew of the *Trusty*, ten in number, one died of cholera at sea; a second on shore at Leith; a third remained there, but recovered; a fourth, whose case proved afterwards fatal, sickened while the vessel lay in Leith Roads. Three others were attacked in St. Margaret's Hope, and sent on board the *Nymphe*; and of these three one reco-

vered and two died. The porter who joined the vessel at Leith also took the disease there, and died. The three remaining members of the crew, and the pilot, who came on board in Dunbar Bay, had each an attack of diarrhoea.

“Of the *five mariners* who acted as nurses to the crew of the *Trusty* on board the hospital ship, one was attacked with cholera during the afternoon of the 15th, and died in less than twelve hours. On the 24th, another of them was attacked with nausea, vomiting, purging, and tenesmus, but recovered. On the morning of the 27th, a third is reported in the ship's journal as attacked with the premonitory symptoms of cholera, but is entered as better on the 28th and 29th. On the evening of the day on which the third nurse was attacked, a sailor was sent on board the *Nymphe* from a second infected vessel that had arrived in the quarantine ground, but up to that day only the men of the *Trusty* had, with the exception formerly noticed, been on board of her as patients.”*

Had the *Brutus* been less severely visited, the captain would, no doubt, have held on to his destined port; and the passengers, for their own sakes, would have spoken of the occurrence of cholera on board their vessel as little as possible, and so the matter would have been hushed up. The occurrence of 81 deaths at sea, among less than 350 persons on board the same vessel, cannot be accounted for, unless on the supposition that the disease is contagious. One such positive fact is worth a volume of negative evidence.

On the 10th of June, 1832, it appeared at Montreal, and here, as at Quebec, it immediately assumed the character of a most destructive pestilence.

The following interesting account† of the route of cholera during the first stages of its progress in North America, is from the pen of S. Jackson, M.D., Secretary to the consulting Medical Board of Philadelphia. Dr. Jackson is a non-contagionist,

* Dr. Simpson's *brochure*.

† *Cholera Gazette*.

as will abundantly appear from his narrative, upon some of the leading facts of which I may hereafter take occasion to make a few observations. It is worthy of remark that the medical men of America have far outstripped their European colleagues in medical statistics. The weekly, monthly, and annual accounts of diseases, deaths, &c. in each of their great cities have been long published systematically and regularly, and that with a degree of accuracy to which we are strangers. Some of the results of this praiseworthy habit appear in Dr. Jackson's account.

“ From the numbers of emigrants, who, about this period, had landed at Quebec, and arrived at Montreal from England and Ireland, a first impression was created, that they had been the means of transmitting the epidemic across the Atlantic. A more close investigation into the facts connected with the commencement of the disease in those cities, served to destroy this supposition. It could not be traced to importation. The emigrants and lower classes of the Canadians were attacked simultaneously in both cities. Numbers of the emigrants were in circumstances eminently predisposing them to suffer attacks of the disease, and they and the lower Canadians were precisely the description of persons most obnoxious to the ravages of epidemic cholera, and such as have been universally observed to be its first victims.

“ The lines of communication between the cities of Quebec and Montreal, and the cities of the United States, are by the Richelieu River, Lake Champlain, and the northern canal leading to Troy and Albany ; or by the St. Lawrence to Lake Ontario, to Buffalo, and by the Erie Canal leading to Rochester and Albany. It was confidently expected that the disease would penetrate into the United States from Canada by these routes. Along the first, many cases of the disease did certainly occur in the persons of emigrants, but they terminated without its communication to others. On the contrary, the epidemic manifested a decided predilection for the shores of the St. Lawrence, suc-

cessively attacking the towns and villages along its banks, then following the borders of Lake Ontario, until it entered Lake Erie.

“ While attention was directed to the northern and western boundary, supposed to be threatened by the invasion of the disease, it suddenly and most unexpectedly appeared in the city of New York.

“ The first case occurred, it is said, on the 24th of June, when a man, a native citizen, residing at the corner of Gold and Frankfort streets, was attacked by the disease. Four cases soon succeeded, the location of which was in Cherry-street. The subjects were Irish emigrants, who had arrived in Quebec in the autumn of 1831, and had resided in Albany until the month of May, when they removed to New York.

“ On the 27th June, the disease manifested itself in Bellvue Alms-house, distant about three miles from the city. The patient was an aged woman who had not left the house for three years, who had held no communication with the city, and no admission into the ward she occupied had taken place for a month. Several cases immediately ensued in this and the other wards of the house. The epidemic reached its maximum in this establishment on the 11th July, and terminated on the 4th August.

“ In the city of New York, the climax of the epidemic arrived on the 11th of July, from which period it continued very steadily to decline.

“ The time that elapsed from the out-breaking of the epidemic at Quebec, and its appearance at New York, is a period of sixteen days, or nineteen at Bellvue Alms-house. The distance between the two cities in a direct line, is four hundred and fifty miles.

“ It is to be remarked that all the intermediate cities on the sea-board of the province of New Brunswick and Nova Scotia, of the states of Maine, Massachussetts, and Rhode Island, remained entirely exempt from the epidemic ; and even to the

present period, except in Providence, Newport, and Boston, no cases have as yet appeared.

“ In this city, the epidemic was much more tardy in its progress than it had been in the Canadas, or in New York. The first decided case of cholera occurred on Thursday, July 5th. A man of the name of Musgrove, residing in the cellar of a house in Filbert-street, near Schuylkill Fifth-street, was attacked with symptoms of malignant cholera on that day. This man had but lately been discharged from the New Jersey prison ; he had been affected with diarrhœa for two or three weeks previous to the cholera symptoms. The disease proved fatal on Sunday the 8th. The next case was a black man residing in St. John-street, Northern Liberties, above Callow hill. He had been employed working on board a ship from England, lying at Pratt’s-wharf. He was seized with symptoms of malignant cholera the night of Tuesday, July 9th, and died on Friday. This man was perfectly sober in habits ; no premonitory symptoms existed.

“ No other cases presented themselves until Sunday, July 14th, when two females occupying a room in a dwelling in Coate’s-street, a few doors above 3rd, were the victims of the pestilence in its most aggravated shape. Both these females were exemplary in their habits of life, but appeared to be infirm in health. The husband of one of these unfortunates had arrived on Saturday, July 7th, from New York, exceedingly alarmed respecting the cholera. He was taken sick the next day, and died on the succeeding Friday. On Saturday the widow felt unwell, and without advice took grains xvi. of calomel in the evening. She was soon afterwards seized with vomiting and purging, and in the course of the night she sunk into collapse. She died Sunday night. The mother of the deceased husband, on Sunday morning complained of feeling unwell, but without definite symptoms. Having been up with her daughter-in-law during the night, her uncomfortable feeling was attributed to fatigue. She was then going about the house, and had been out on an errand. She was requested to lie down as a matter of

of precaution, and a small dose of opium administered to her. This was at 8 o'clock in the morning. Dr. Schott, who was in attendance an hour afterwards, went up to her chamber to inquire into her state. He found her lying on the floor; copious dejections of rice-water looking fluid had occurred, and she was in complete collapse: death ensued in the evening. These were the only cases to which the slightest suspicion of communication by contagion could attach; but on the same day a French woman, temperate in habits, about fifty years of age, living in Kensington, beyond the close built part of the town, at the head of West-street, was also a victim of the disease. This woman had not been from her dwelling for three weeks; the house is isolated, being surrounded by kitchen-gardens for the supply of the market. She had been affected with diarrhœa since Friday, for which she had dieted, but had taken no medicine: the case proved fatal next day.

“ From this time not more than three or four cases occurred, all scattered in different quarters, particularly Kensington, Northern liberties, and Southwark, until the 27th and 28th July, when the epidemic fairly set in, and cases continued daily to be developed. The disease attained its height in this city on 5th, 6th, and 7th August, since which time it has gradually declined, and appears now to be extinct.

“ Taking the 27th or 28th of July as the proper commencement of the epidemic in Philadelphia, there will be a period of twenty-four or twenty-five days intervening between its first appearance in New York, and this city. The distance in a direct line is about ninety miles.

“ A comparative view of the population, number of cases and deaths in the cities, which have been brought under observation, presents the epidemic in an interesting point, and exhibits in a clear manner the character it assumed in this city.

Date of Report and Place.	Popula- tion.	Cases.	Deaths.	Ratio of Cases to Popula- tion.	Ratio of Deaths to Cases.	Ratio of Deaths to Population.
Sept. 30, Quebec . .	32,000*	5783	3292†	1 in $5\frac{1}{7}$	1 in $2\frac{1}{2}$	1 in $10\frac{1}{2}$
„ 1, Montreal .	28,000‡	4385	1853	1 in $6\frac{1}{2}$	1 in $2\frac{1}{2}$	1 in $15\frac{1}{9}$
Aug. 22, New York .	140,000§	5547	2782	1 in $25\frac{1}{3}$	1 in 2	1 in $15\frac{1}{2}$
Sept. 13, Philadelphia	160,000¶	2314	935	1 in 70	1 in $2\frac{1}{2}$	1 in 173 29-183

“ The results of this table show conclusively, that the causes productive of cholera were less numerous in the city of Philadelphia than in Quebec, Montreal, or New York, or were so modified as to possess a much less degree of activity. The causes of this result, so favourable to Philadelphia, important in the hygienic history of cholera, and consoling to humanity, as placing this formidable affection to so great an extent under control, it is interesting to investigate.

“ The following are the circumstances which, existing more particularly in Philadelphia, may be regarded as influential in ameliorating the violence of the epidemic cause, circumscribing its activity, and diminishing its fatality.

“ 1. The plan on which the city is built, arranged in hollow squares, separated by wide and paved streets, prevents excessive crowding of inhabitants, procures free ventilation, and gives facility to the means of cleanliness. It is to be regretted that any deviation has been permitted in the original design of Penn, whose sagacity and foresight have been so amply demonstrated in the circumstances of the late epidemic.

“ 2. The abundant supply of wholesome water placed at the command of the whole community, affords a healthful beverage,

* “ Permanent population, 27,000 ; transient population, 5000.—Total, 32,000.

‡ “ Permanent population, 25,000 ; transient population, 3000.—Total, 28,000.

§ “ Estimated as remaining by Mr. D. Leslie.—*Journal of Commerce*, Aug. 8th.

† “ Protestant grounds, 1244 ; Catholic cathedral, and cholera grounds, to 25th September, 1574 ; at St. Roch, 470.—Total, 3292.

|| “ Report of the Inspector.

¶ “ Population within the bill of mortality.”

and gives the means of the most complete cleanliness, by washing the dirty gutters of the streets, close alleys, and lanes.

“3. The well arranged measures of sanitary police, devised and actively carried into effect by the councils of the city, and the boards of commissioners of the district, and the sanitary committees appointed by them, and by the Board of Health. The measures consisted in a thorough investigation into all existing nuisances, and in their immediate abatement; in a complete system of cleanliness of the city steadily pursued; in the early establishment of numerous local hospitals, provided with ample medical attendance, nurses, and every means applicable to the treatment of the disease; and in spreading before the public early information, derived from the consulting medical committees, of the methods, hygienic, dietetic, and medicinal, best adapted for guarding against the attack of the disease, or to arrest the symptoms on its onset.

“4. A very considerable influence may be attributed to the annunciation made by the mission sent to Canada, immediately on its return, and before the epidemic had commenced its career in this city, of the different periods of the disease, and especially of the existence, in almost every instance, of premonitory signs, and a preliminary stage, with a description of the symptoms indicating its existence. This information was communicated to the public by the sanitary committee through the daily journals of the city, by handbills liberally distributed, and by placards on the corners of the streets. The Board of Health adopted the same measures, and pursued the same course. In this manner the whole community, before the beginning of the epidemic, were instructed in the most important points in the general knowledge and management of this affection—its commencing period, the premonitory symptoms, its general curability in that state, the necessity of immediate attention and medical advice, and the methods of relief. These facts had been overlooked, and this attention to the instruction of the public were entirely neglected in Quebec, and Montreal, and in New

York ; from being taken unprepared by the epidemic, earlier than was anticipated, they were not communicated to the public until the measure had been adopted in this city, and when the epidemic there had already attained its maximum of intensity.

“5. The moral resolution, calmness, and perfect freedom from alarm and panic, generally manifested by our citizens, and inspired by a thorough confidence in the efficacy of the preventive means enforced, in the advantages for salubrity of the city, and in its medical resources, contributed in no small degree to diminish the number of cases, and the intensity of the attacks. No stores were closed on account of the epidemic, and not more citizens left the city than usually abandon it every summer. A stranger entering our streets, from the busy throng and cheerful aspect of all he met, would never have suspected the existence of an unusual and a desolating scourge.

“6. The treatment of the disease generally pursued in the city, in the preliminary stage, had most probably no small share in preventing the development of the disease in innumerable instances. In the lighter forms, it was limited chiefly to diet, rest, tranquillizing doses of anodynes, or mild diffusibles, with occasionally the mildest laxatives or gentle cathartics, conjoined with sinapisms or other rubefacients. The drastic and perturbing cathartics were seldom if at all prescribed, and the stimulant practice but rarely resorted to.

“The foregoing circumstances appear to us as those principally instrumental in producing the favourable results attending the epidemic in this city. As such they acquire a high degree of interest, and afford most instructive lessons as regards the measures of municipal and civil regulation connected with sanitary police.

“In its general features and character, the disease differed in no respect from the many descriptions that have been made since it first attracted attention in Asia, and subsequently in its progress through Europe. It will be unnecessary to make the repetition here ; it is, however, important that the fact should be

signalized, that during the prevalence of the epidemic, very few persons in the city were entirely exempt from some derangement or disorder of the digestive functions. It is not probably exaggeration to assert, that two-thirds of the population were affected in this manner, which is to be attributed entirely to the epidemic influence. It should also be stated, that in the majority of cases which assumed the decided character of malignant cholera, preliminary symptoms had existed, varying in duration from a few hours to several days. In those rarer instances which were not preceded by any premonitory signs, the subjects were the aged, the intemperate, individuals who had committed some great imprudence in diet, or whose constitutions had been enfeebled, and such cases were generally, if not universally, fatal.

“ The disease was not confined to any one portion of the city, but extended to every district. Neither did it progress gradually from one quarter of the town where it first appeared to others, but broke out almost simultaneously in the most opposite and distant points.

“ The following table exhibits the number of cases that were reported from the different districts, with the ratio to the population.

	Population.	Cases.	Ratio of Cases to Population.
City	80,458 .	407 .	1 in 197 7-8
Kensington, . . .	13,320 .	111 .	1 in 120
Northern Liberties	28,932 .	144 .	1 in 200 11-12
Penn Township . .	11,141 .	55 .	1 in 202 3-7
Southwark	20,740 .	251 .	1 in 82 4-28
Moyamensing . . .	6,822 .	198 .	1 in 39 5-11

“ From the above table it appears that the epidemic prevailed with greatest severity in Moyamensing and Southwark. This is to be attributed to the character of the population, rather than to local causes. In both these districts reside the worst portion of our population, and in Moyamensing, espe-

cially, there is a dense population, some of whom are of the lowest order and most abandoned habits.

“ In the city, though the cases as occurring in different parts, were not kept distinct in the reports, yet it is well known that the larger proportion of them took place in the external limits, especially the western borders, towards the Schuylkill, and the southern extremity, while a very small number only were developed in the central portion.

“ The chief mortality of the disease existed in the public institutions. It was much lighter in private practice. The following Table exhibits the cases of deaths, as reported in private practice, and the public institutions. The reports, however, do not exhibit the results of private practice in as favourable a light as they really were. A considerable number of physicians in the more respectable practice, reported only the cases that proved fatal, or exceedingly severe. They did not return to the Board of Health the lighter cases, which yielded to the operation of remedial measures. The mortality of private practice in the reports, appears, in consequence, to have been far greater than it really was.

“ *Table of Cases and Deaths, with the Ratio as occurring in Private Practice, and the Public Institutions.*

	Cases.	Deaths.	Ratio of Deaths to Cases.
Private practice, . . .	1175 .	270 .	1 to 4 3-16
Hospitals, . . .	874 .	342 .	1 to 2 5-9
Alms-house, . . .	174 .	92 .	1 to 1 41-46
Arch-street Prison, . .	86 .	46 .	1 to 1 20-23

“ Had the returns of cases in private practice been complete, the proportion of cases would have been much greater, it would have ranged probably as 1 to 70 or 80, or even more.

“ In the hospital practice, the first cases introduced were nearly all fatal. This circumstance is to be accounted for from the universal observation, wherever cholera has prevailed epidemically, that the worst constitutions were the first to suffer

attacks. In the commencement of the epidemic, persons first attacked, unaware of their danger, and the nature of the affection, neglect application for aid, and resist the offer of hospital assistance until reduced to a hopeless condition. Besides, misled by the authority of the English and Scotch writers, extensive means had been prepared for warming the patients by heated air, steam, and other means. Experience in a short time proved the pernicious effects of this system. The patients succumbed most rapidly under the exhaustion induced by the profuse watery exhalation from the skin caused by this treatment.

“ The disease first appeared in the Alms-house, July 29th ; it reached its period of greatest activity on the 8th and 9th of August, gradually declined, and terminated on the 25th of August.

“ In the Arch-street prison are confined vagrants, disorderly persons, criminals guilty of petty larceny, most of them the victims of low and brutal debauchery, and a limited number of debtors.

“ The disease manifested itself on the 31st of July. Cases continued to occur daily, but on the 5th of August, the number of cases and deaths suddenly augmented, producing a scene of almost unexampled desolation. In the same room were mingled the dead, the dying, the sick, and the well. The prisoners became frantic with despair, and threatened the lives of the officers and attendants. A number of medical gentlemen, the inspectors and others, repaired to the prison to alleviate the sufferings of these unhappy beings. The vagrants were discharged, the sick were conveyed to the hospitals, and all the prisoners whom it was possible to release were dismissed. The confusion was so great, that a return of the cases and deaths was not made to the Board of Health on that day. By reference to the meteorological table, it will be seen that on the 5th August, the day the disease in the prison acquired its sudden intensity, the barometer had fallen lower than it had been for a month previous, the maximum of the thermometer was at the highest point

for the month, and the dew point at the highest elevation. The atmosphere in consequence was light, moist, and oppressive to the feelings. Was this meteorological state of the atmosphere, and the sudden augmentation of the disease, mere coincidence, or were they connected?

“The mortality of the disease in relation to sexes, is shown in the following Table. The relation as to cases cannot be ascertained.

“Number of deaths from commencement of cholera to September 1st, per weekly reports of interments, was—

Deaths, 909.	Males, 539.	Females, 370.
Under 20 years.	do. 70.	do. 48.

“*Table of Deaths from Cholera, arranged as to Periods of Life; showing also the Ratio of Deaths from Cholera to the Periods of Life.*

Ages.	Deaths.	Ratio.
Under 1 year	4	1 in 604
Between 1 and 2 years	4	1 in 503
2 5	30	1 912
5 10	39	1 919
10 15	19	1 188
15 20	22	1 96
20 30	179	1 81
30 40	228	1 60
40 50	159	1 46
50 60	100	1 28
60 70	71	1 102
70 80	47	1 212
80 90	5	1 36
90 100	1	
100 110	1	

909

“From this Table it results that the earlier periods of life give the greatest exemption from the attacks of the disease, especially the ages from 2 years to 10 years; and that the

period of life most prone to be affected, is from 40 to 60 years, and more particularly from 50 to 60 years.

“The ravages of the disease were more extensive in the coloured than in the white portion of the population, in proportion to numbers. The fact is shown in the following:—

“ *White Population.*

Nineteen hundred and seventy-seven cases.

Ratio of cases to white population—1 to 74.

“ *Coloured Population.*

Three hundred and thirty-eight cases.

Ratio of cases to black population—1 to 41.

Ratio of blacks to white population—1 to 11 4.7.

Ratio of cases of blacks to whole number of cases—1 to 6.

“It has been a common observation by writers on epidemic diseases, that during the prevalence of an epidemic, it appeared to subdue and suppress all other diseases, monopolizing to itself, for a time, all the energies of destruction. This observation has been repeated since the days of Sydenham, by whom it was announced, though it has not been supported by statistical evidence.* In the present epidemic, although its influence was so extensively felt in the city, the observation has not been sustained. The following Table shows very clearly, that during the prevalence of the late epidemic, other diseases continued, not only unabated, but actually augmented, causing an increase of mortality independent of that produced by cholera. During the months of June, July, and August of this year, the deaths from the diseases generally prevalent, exceeded those of the corresponding months of last year, 425. It is to be remarked, however, that the diseases in which the augmentation of the mortality was the greatest, are those congenerous with cholera, viz. gastric, enteritic, febrile diseases, and inflammations. All those

* This opinion is also supported by Villermè who says, that during the cholera epidemic in Paris, other diseases became more rare. I prefer Dr. Jackson's view of the case, as agreeing with my own experience during the cholera epidemic in Dublin in 1832 and 1834.

diseases appear to have derived an increase from its presence. It is also to be observed, that scarlet fever, instead of yielding to the sway of cholera, was actually augmented.

“ Table showing the prevailing Diseases independent of Cholera ; what Influence it exerted over them ; and the Rate of their Mortality.*

DISEASES.	1831.				1232.			
	June.	July.	Aug.	Totals.	June.	July.	Aug.	Totals.
Consumption,	35	41	33	109	44	52	73	169
Convulsions,	18	26	29	73	28	29	39	90
Cholera Infantum . .	45	132	82	259	25	134	157	316
Diarrhœa and dysentery,	18	28	49	95	15	47	83	145
Fevers,	17	24	35	76	31	35	65	131
Scarlet fever,	5	9	10	24	23	17	14	54
Inflammations in general,	32	19	26	77	28	43	29	100
Inflammations in the chest,	16	10	8	34	16	15	7	38
Inflammations in the abdomen,	16	9	18	43	12	28	22	62
Dropsy in the head, .	22	22	29	73	5	33	23	61
Do. in the chest, .	2	4	6	12	2	4	3	9
Do. in general, . .	6	12	11	29	3	10	9	22
Debility and decay, .	28	33	29	90	16	45	28	89
Apoplexy,	9	8	4	21	4	8	7	19
All diseases, (still born deducted,)	294	467	490	1251	369	785	1431	2585
All diseases, (malignant cholera deducted,) .	294	467	490	1251	369	689	618	1676
Excess in mortality of 1832,					75	318	941	1334
Excess after deducting mortality from cholera,					75	222	428	425”

Why the cholera, *if an imported disease*, should have broken nearly out simultaneously in Quebec and Montreal, is very easily accounted for, since both are the receptacles of British and other foreign emigrants ; and on the same principle, we must explain its appearance so soon after at New York, where, no doubt, it arrived by a separate importation from Europe, a circumstance which will prevent us from feeling the same sur-

* “ For this Table I am indebted to Dr. Emerson.”

prize with Dr. Jackson, that between Quebec and New York *all the intermediate cities on the sea board escaped*, at least for a few months. This is analogous to the exemption of Waterford and Wexford during several months that cholera raged in Dublin and Cork. I have published Dr. Jackson's Report unabbreviated, because it is intended to be conclusive against the theory of contagion; while it, in my opinion, contains strong internal evidence of a contrary tendency. The great number of cases which occurred in the Alms-house and Arch-street prison, seems to me to furnish a conclusive argument in favour of its contagiousness; and Dr. Jackson admits that it arose among the lower orders of emigrants both in New York and Philadelphia; but as I do not wish to entangle myself in a protracted discussion of this question, I shall now resume the account of the progress of cholera. In the United States it spread far and near, as might be expected from the wonderfully rapid and frequent intercourse that takes place all over the Union; but except in the condensed population of the chief sea-ports, its ravages were not great. It is curious to observe how little Philadelphia suffered in comparison with Montreal, Quebec, or New York; no doubt because its population is less condensed, and live in families more separated from each other. In making this observation, I do not mean to undervalue the power of predisposing causes, such as poverty, bad diet, intemperance, &c., which prevail more in the latter cities than Philadelphia. Still, comparing America with those European and Asiatic countries which suffered most, the only constant difference we can discover is, that the separation of families is much more complete in the United States than in any other country except England; and to this difference, consequently, we are justified in referring for an explanation of the remarkable fact, that England and the United States fared better than other countries, notwithstanding their acknowledged superiority, above all in the facilities of internal communication. A wish to be brief forces me to conclude the subject of the cholera in

North America, with the following list of places, and the dates of its arrival in each.

Albany,	3rd July, 1832.
Troy,	16th July, „
New Brunswick,	July, „
Rochester	July, „
Baltimore,	August, „
Washington,	August, „
Boston,	August,* „

Cholera did not reach South America at all, a fact explicable by the great length of the voyage from the infected countries, which reason also protected the Cape of Good Hope, the West Indies, and New Holland. It is a curious fact, that New Holland, for the same reason, has hitherto been free from measles, scarlatina, and hooping-cough, although the colony is fifty years old. We must now return to Europe; and first with respect to Portugal. It appears from the following editorial paragraph in the Medical Gazette,† that the disease was imported. “The London Merchant Steamer sailed from England for Oporto, on the 25th December, 1832, and arrived at the mouth of the Douro on the 1st January, 1833, having lost seven persons on her passage by cholera. The troops which she took out, with General Solignac, landed immediately at Foz, about two miles to the west of Oporto. By a letter from a medical gentleman of that city, which we have lately seen, it appears that cases of the disease occurred at Foz, on the road to, and in Oporto, before the 15th of January; and we know from other authorities, that it has since spread to *Coimbra* on the south, and *Vigo* on the north.”

Mr. Lardner, a very intelligent surgeon, and formerly a

* I am not certain of the dates of its first appearance where the day of commencement is not mentioned; but in all the above places the cholera prevailed during the above months.

† Vol. xii. p. 123.

pupil of mine, has written a very interesting paper on the progress of cholera in Portugal.—Lancet, 1834-5, p. 314. He is a decided non-contagionist, but his facts seem to me to be strongly corroborative of the doctrine of contagion. Among other admissions, the following is almost conclusive. “Lisbon was not visited by cholera for a considerable time after Aveiro; which fact may give the contagionist a lift, for during the siege there existed no direct communication by water between Oporto and Lisbon. The Miguelite batteries would not allow a ship to enter the Tagus, and Donna Maria’s ships kept a strict blockade outside the bar.” The epidemic took six months to travel slowly by land from Oporto to Lisbon. Had the communication by sea between those two ports been open, no doubt it would have reached Lisbon sooner: in America how quickly it extended from one seaport to another. It is a remarkable circumstance, and one which ought to have great weight in the discussion respecting the contagiousness of cholera, that *cholera has in no recorded instance appeared in any place sooner than the ordinary modes of communication might have brought it from some infected station*. Again, it can easily be proved that *the rate at which cholera travels varies with the rapidity of that communication*. A few weeks were sufficient to transport it from the ports of Britain more than three thousand miles across the Atlantic to Canada, while it took six months to creep along the interrupted line of communication between Oporto and Lisbon. The following dates refer to Portugal, Spain, and Italy. I regret that I have not ascertained more numerous and accurate *data* respecting its progress in these countries, but what I have been able (in my hurry) to lay my hands on, will perhaps prove sufficient for my present purpose, which is only to map out the route and march of the main body of the disease. From the preceding observations it will appear, 1st, that cholera has had no fixed rate of progress; 2nd, that it has spread in every direction, sometimes travelling northwards, sometimes southwards, at other times east or west, its route

being determined not by the points of the compass, but by the great lines of internal and international communication.

Oporto,	3rd January, 1833.
Aveiro,	3rd February, „
Coimbra,	March, „
Vigo,	March, „
Lisbon,	15th June, „
Havannah,	26th Feb., „
Drammen, (Norway,) . . .	1832.
Christiana,	29th September, 1833.

Cholera never got to any other West Indian islands, nor to British (formerly Dutch) Guiana, Demerara, nor any of the embouchures of the great South American rivers, Amazons, Orinoco, or La Plata—[Dr. Gilgeous]—though the soil and climate, with the immense tracts of inundated and swampy lands, would there seem most favourable to its development.

Cholera spread extensively in France in Summer :—

Marseilles,	Spring, 1835.
Toulon,	ditto, „
Mexico, (frightful,) . . .	Summer, July, 1833.

In September, 1835, (*Lancet*, vol. for 1834-5, p. 782,) “ the cholera had nearly ceased its ravages in the South of France, and took a south and easterly direction along the countries bordering the Mediterranean Sea. It has penetrated into Piedmont in spite of the strictest precautions, and now prevails with more or less intensity at Nice, Coni, Livorno, Genoa, Florence.” From this extract we do not learn the dates of its arrival at the above places, but they were probably according to their respective distances from France. The kingdom of Naples was not infected until a still later period : at Naples, probably, September, 1836. It attained the maximum at Naples on the 22nd November, 1836 ; Algiers, 14th October, 1837 ; Bona, September, 1837.

To trace it accurately, its secondary routes and dates of reappearance should be made out ; it would then be found to have returned often on its steps.

Thus in September, 1837, Marseilles was attacked for the third time, while in same season of the year 1837, it reappeared also at Berlin, Prague, and Danzig.

It is worthy of remark, that cholera began at Naples, which carries on a perpetual commercial intercourse with Marseilles, about a year before it commenced in Rome ! August, 1837. The disease travelled southwards in the North of Italy, setting out from France ; northwards in the south of Italy, starting from Naples.

An interesting question here arises, whether cholera is likely to become a permanent resident in this country. Its history indicates that it will not ; for although cases of Asiatic cholera are now and then met with, they are by no means so violent or intense as formerly ; and their comparative unfrequency may be judged from the fact, that but 460 deaths from cholera took place in England and Wales during the six last months of 1837, while other diseases formerly introduced from abroad, but which have become thoroughly naturalized in Great Britain, present a very different result. Thus in the half year the deaths from

Small-pox were 5,811.

Measles 4,732.

Scarlatina 2,520.

Sir James Clark, Physician to her Majesty, has interested himself much in obtaining information for me respecting the progress of cholera in Great Britain ; and has on this occasion, as on many others, displayed a most praiseworthy zeal for the promotion of medical science. To him I am indebted for maps of Scotland, England, and Ireland, constructed specially for my use, and in them are marked, with the greatest accuracy, all the places where cholera appeared. These maps I had

intended to publish, but found that the expense would be very considerable, and far beyond what could be borne by a periodical; as a substitute for the maps, I have composed a list of every city, town, or village attacked by cholera, in Great Britain; and have arranged the names in counties beginning in the north of each kingdom, and setting down the counties, as we proceed southwards, in an order arranged (in each successive set) from west to east. This list will enable any of our readers to construct the maps for himself with great ease and accuracy. The numbers in the list refer, I believe, to the order of succession in which the disease arrived at each place respectively. If this be so, the value of the Table will be still greater. Sir James Clark has most kindly promised to procure the precise dates for every place as soon as possible, and I shall not fail to publish them in the March Number of this Journal.

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Boulsworth, 90
Bradford, 167
Leeds, 83
Halifax, 178
Dewsbury, 182
Wakefield, 103
Rawdon, 99
Barnsley, 98
Cawood, 86
Micklefield, 57
Barlby, 92
Howden, 79
Kingston, 62

Swinefleet,	55	Lincoln,	126
Snaith,	84	Newark,	117
Ferrybridge,	89	Sleaford,	154
Bentley,	126	Donington,	176
Doncaster,	11	Swineshead,	119
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Sheffield,	119	Newton,	193
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DENBIGH.		Wellington,	175
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Northwich,	81	Bilston,	140
Chester,	120	Bridgenorth,	144
Bunnington,	185	Dudley,	51
Stockport,	100	Oldbury,	123
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Attercliff,	124	Moseley,	150
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Rasford,	86	Stourport,	120
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St. Ives,	69	Henly,	171
Ramsey,	66	Wattington,	81
CAMBRIDGE.		BUCKINGHAM.	
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Ely,	45	Blackthorn,	86
Standground,	82	Aylesbury,	98
NORWICH.		Brill,	104
Lynn,	59	Haddington,	160
Houghton,	109	Stoke,	135
Cawston	77	Mandeville,	150
Downham Market,	100	Rickmansworth,	56
Stokeferry,	100	Hambleton, or Great	
Norwich,	155	Marlow,	164
Yarmouth,	46	HERTFORD.	
SUFFOLK.		Hertford,	81
Mildenhall,	164	Ware,	116
Woodbridge,	185	Watford,	131
PEMBROKE.		ESSEX.	
Haverford West,	102	Waltham Abbey,	136
GLAMORGAN.		Edmonton,	187
Swansea,	144	Chelmsford,	97
Neath,	200	Billericay,	114
Aberdar,	203	Barking,	167
Abarafon,	137	Rochford,	110
Merthyr Tidvill,	162	CORNWALL.	
MONMOUTH.		Trenbetha,	163
Abervageny,	213	Penzance,	177
Newport,	116	St. Paul's	126
GLOUCESTER.		Cumborne,	183
Gloucester,	119	St. Breock,	152

Padstow,	143	Clifton,	150
Callington,	160	Tiverton,	150
Liskeard,	197	Bath,	134
St. German's,	148	WILTSHIRE.	
Saltash,	139	Chippenham,	56
Cawsand,	135	Hungerford,	189
DEVON.		BERKSHIRE.	
Tavistock,	158	Wantage,	184
Handmonnachoram,	?	Englefield	151
Ashburton,	149	Windsor,	176
Meavy,	134	MIDDLESEX.	
Devonport,	118	Uxbridge,	127
Plymouth,	96	Brentford,	?
Plympton,	137	London,	17
Harberton,	168	Tottenham,	132
Totness,	139	Edmonton,	187
Kingsbridge,	157	DORSET.	
Dartmouth,	139	Bridport,	151
Brixham,	166	HAMPSHIRE.	
Chudleigh,	153	Cowes,	213
Kenton,	150	Portsmouth,	132
Topsham,	160	KENT.	
Exmouth,	145	Bromley,	132
Withcombe,	165	Farmingham,	186
Brittan,	146	Greenwich,	?
Alphington,	136	Woolwich,	?
Exeter,	126	Dartford,	83
Otterton,	196	Cobham,	119
Clist,	139	Maidstone,	162
Honiton,	153	Gravesend,	?
SOMERSET.		Chatham,	?
Taunton,	191	Milton,	48
Wells,	193	Queensborough,	134
Paulton,	187	Sheerness,	94
Oldland,	169	Dover,	101

IRELAND.

DONEGAL.

Stranorlan,	118
Inver,	168
Donegal,	152
Drumholm,	?
Lifford,	108
Ramelton,	6
Castle Carey,	158

LONDONDERRY.

Londonderry,	90
Coleraine,	86
Kilelagh,	89
Magherafelt,	63

ANTRIM.

allymenagh,	116
Bandalstown,	72
Larne,	166
Antrim,	53
Belfast,	1

FERMANAGH.

Kilbarron,	84
Ballyshannon,	98
Belleck,	90
Enniskillen,	86

TYRONE.

Ardstra Bridge, . . .	147
Strabane,	87
Newtown Stewart, . .	102
Cappagh,	132
Omagh,	101
Clogher,	148
Aughnacloy,	123
Dungannon,	58

CAVAN.

Tamregan,	154
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Killishandra,	141
Kilmore,	140
Castleterra,	151
Belturbet,	150
Kilersherding,	24

MONAGHAN.

Monaghan,	67
Ballybay,	159
Clones,	10
Carrickmacross, . . .	110

ARMAGH.

Charlemont,	160
Blackwatertown, . . .	142
Lurgan,	66
Tanderagee,	67
Armagh,	76
Fork Hill,	12

DOWN.

Moir,	148
Hillsborough,	63
Dromore,	63
Killileagh,	89
Bangor,	67
Grey Abbey,	125
St. Andrew's,	95
Ballyphilip,	105
Saul,	125
Downpatrick,	81
Banbridge,	61
Kilkeel,	57
Rathfriland,	134
Clonallen,	9

MAYO.

Killala,	103
Ballina,	91

Burrishooll,	38	MEATH.	
Tamore,	115	Athboy,	129
Swineford,	115	Navan,	15
Castlebar,	49	Kells,	13
Oghaval,	67	Trim,	39
Kilcommon,	112	Slane,	15
Ballinrobe,	47	Rathmolion,	99
Kilmain,	31	Duleek,	67
Cong,	134	LOUTH.	
SLIGO.		Flurry Bridge,	53
Cooloney,	82	Carlingford,	8
Ballisadare,	93	Dundalk,	13
Sligo,	83	Castlebellingham,	122
Ahamplish,	156	Ardee,	18
LEITRIM.		Dunleer,	19
Drumkeer,	172	Termonfeekin,	24
Kilboghhard,	175	Drogheda,	17
Carrick,	173	DUBLIN.	
Mohill,	157	Balbriggan,	25
ROSCOMMON.		Skerries,	36
Boyle,	87	Ballyboghil,	16
French Park,	70	Lusk,	75
Elphin,	114	Rush,	79
Tarmonbarry,	137	Malahide,	16
Roscommon,	130	Portmarnock,	80
Ballynullalo,	78	Baldoyle,	32
Athleague,	148	Howth,	10
LONGFORD.		Santry,	63
Clongesh,	132	Clontarf,	12
Longford,	55	Hollywood,	65
Granard,	134	Castleknock,	75
WESTMEATH.		Glassnevin,	33
Castlepollard,	158	Finglass,	66
Kinnegad,	104	Chapelizod,	33
Athlone,	37	Palmerstown,	155
Kilbeggan,	145	Clondalkin,	4

Dublin,	2	Nass,	5
Rathfarnham,	17	Rathangan,	16
Newcastle,	44	Newbridge,	131
Stillorgan,	17	Athy,	22
Monkstown,	51	Castledermot, . . .	140
Blackrock,	57	WICKLOW.	
GALWAY.		Bray,	51
Galway,	22	Powerscourt, . . .	120
Tuam,	42	Dunlavin,	15
Kilascobe,	147	Arklow,	5
Athleague,	148	KILKENNY.	
Ballinamore,	162	Donaghmore, . . .	91
Ballinasloe,	15	Freshford,	169
Athenry,	62	Kilkenny,	84
Gort,	36	Thomastown, . . .	87
Loughrea,	66	Innistioge,	178
Kilbride,	150	Gowran,	151
Eyre Court,	48	Callen,	44
Portumna,	87	Kilmurry	41
KING'S COUNTY.		Carrick,	65
Tullamore,	19	CARLOW.	
Lynally,	46	Tullow,	85
Portarlington, . . .	122	Carlow,	16
Kilcoleman,	87	Leighlin Bridge, . .	109
Castropeter,	64	Bagnal's Bridge, . .	105
QUEEN'S COUNTY.		Gore's Bridge, . . .	135
Mountmellick, . . .	53	Graigne Namanna, .	11
Anatrim,	59	WEXFORD.	
Maryborough,	63	Ross,	118
Durrow,	148	Taghmon,	147
KILDARE.		Enniscorthy,	127
Maynooth,	69	Ferns,	95
Celbridge,	68	Wexford,	87
Springfield,	68	CLARE.	
Straffan,	23	Killfenora,	66
Kill,	80	Inistymon,	67
Killibegs,	160	Dromcliffe,	47

Ennis,	40	Emly,	169
Cloney,	417	Clonmel,	50
Tullow,	34	Fethard,	122
Skariff,	56	KERRY.	
Killaloe,	63	Dingle,	91
Killmurry,	41	Tralee,	28
Killeimer,	37	Listowel,	90
Newbridge,	76	Ballylongford,	117
Kinnaleese,	46	Tarbert,	68
O'Brien's Bridge,	98	Killarney,	86
LIMERICK.		Kenmare,	148
Newcastle,	11	Tapsista,	164
Loughill,	54	CORK.	
Askeaton,	42	Ballydehob,	88
Rathkeale,	67	Bantry,	48
Croom,	91	Baltimore,	119
Adare,	96	Castletownsend,	157
Loughmore,	68	Skibbereen,	62
Castle Connell,	47	Ross Carberry,	73
Limerick,	30	Dumanway,	123
Abingdon,	154	Ballyneen,	157
Caerconlish,	159	Clonakilty,	30
Pallasgreen,	170	Kilmurry,	92
Bruff,	88	Crookstown,	92
Bruree,	177	Bandon,	29
Killmallock,	62	Ballinspittle,	100
Glenbriggane,	84	Kinsale,	20
TIPPERARY.		Innishannon,	66
Templemore,	98	Ballymartle,	48
Thurles,	88	Millstreet,	150
Lorrah,	133	Kanturk,	73
Uskean,	107	Buttevant,	16
Nenagh,	67	Doneraile,	95
Kilvellan,	67	Mallow,	22
Golden Bridge,	132	Castletown Roche,	140
Cashel,	73	Kildorrery,	101
Tipperary,	45	Glanworth,	132

Kilworth,	113	Youghal,	68
Fermoy,	48	WATERFORD.	
Castle Lyons,	83	Tallow,	61
Rathcormack,	70	Lismore,	156
Cork,	5	Dungarvon,	55
Passage,	94	Ringmount,	163
Cove,	18	Rossmore,	178
Middleton,	17	Portlaw,	161
Cloyne,	78	Tramore,	111
Ringabella, and Cork		Waterford,	21
Head,	23	Duncannon,	112

The maps from which the preceding Tables have been constructed furnish, even on a cursory examination, some very interesting particulars. In Scotland nine-tenths of the extensive regions north of the Clyde, and popularly termed the Highlands, escaped, and the parts which were visited, form two leading groups, the smaller situated at either side of the eastern embouchure of the Caledonian Canal ; the larger including the country immediately north of the line joining the Firth of Forth and Firth of Clyde.

Now there is here an evident connexion between the visitation and the chief routes of commerce and communication, a fact rendered still more striking by the total immunity from cholera which the whole of the western Highlands enjoyed, there being no infected locality between Inverary and Cape Wrath. Again, the Mull of Cantire escaped, with the exception of Campbleton, a seaport possessing a frequent steam communication with Glasgow. In the Lowlands, the greatest number of infected places occurred in the vicinity of the great line of communication connecting Edinburgh, Glasgow, and Greenock, and near the English borders, particularly towards the eastern extremity, not far distant from the county in which cholera first appeared in England. A very striking circumstance is the freedom from cholera enjoyed by the Western Isles, (Hebrides,) and all the western ports of Scotland, while so many of the eastern ports

were affected. Here the exemption cannot be attributed to any comparative salubrity of climate, and freedom from low, swampy tracts of country, and river courses, but to the manifest facility which the coast trade on the eastern side of the Caledonian peninsula afforded to the transmission of the disease. Many similar deductions might be made from an examination of the map of cholera in England and Ireland, but want of space prevents me from entering into further particulars.

I cannot conclude, however, without noticing the curious and remarkable fact, that the country, which of all others most abounds in swamps, and pestilential miasmata, I mean the western coast of Africa, escaped the visitation of cholera altogether; not a single town on the Atlantic shore of Africa was visited. Sierra Leone, and all the low, half-inundated but thickly inhabited countries in the embouchures and deltas of the Gambia, the Niger, and the Zaire, escaped. Contrast this immunity with the ravages the cholera made in all the Mediterranean seaports of Africa, where the soil is comparatively dry and sandy, and the inference is obvious.

ART. XVII.—*Researches on Operative Midwifery*. No. 3. *The Vectis, or Lever*. By FLEETWOOD CHURCHILL, M.D., Physician to the Western Lying-in Hospital, and Lecturer on Midwifery at the Richmond Hospital School of Medicine, Dublin.

[Read at a Meeting of the Obstetrical Society, on Thursday, Nov. 7, 1839.]

“Sat cito si sat bene.”

MR. PRESIDENT,

As an introduction to the immediate subject of this paper, perhaps I may be permitted to make a very few general remarks upon the classes of cases requiring instrumental assistance.

Natural labour may be defined as the equable adaptation of

the expulsive force, the body to be transmitted, and the passages to be traversed, the one to the other. In other words, it is necessary that the passages and the child should correspond in size, and that the uterine power should be adequate to expel the child within a given time, each stage having a certain relation to the entire duration.

A deviation from the normal proportion in the power, (the uterine contractions,) or in either of the two conditions, (the passages or child,) constitutes unnatural labour, (the preternatural labour of some authors,) and will demand our assistance ; but the kind and degree of aid required will depend upon the amount of the relative disproportion. It is easy to see, for example, that if the passage be unusually narrow, or the head unusually large, that some mechanical contrivance will be necessary to complete the labour. This, I say, is evident, because it is simply a question of measurement.

A comparison of the diameters of the child's head with those of the pelvis, will show us what sized head can pass, and what can not ; and although in practice we cannot very easily ascertain the exact number of inches, yet as the head and the pelvic outlet are in immediate apposition, their relative sizes may be estimated without much difficulty.

In other cases where the disproportion is much less, the difficulty may be increased, inasmuch as the question will probably be whether, allowing for the moulding and compression of the child's head, the natural power may not be sufficient to the delivery ; or if not quite adequate, yet with the least possible help.

But if we turn to the class of cases where the abnormal deviation from natural labour is caused by the inefficiency of the uterine efforts, we shall find the difficulty much greater. We have no standard for ascertaining the adequacy of the pains, nor for deciding upon the necessity of interference, except that which long experience gives to individuals. Besides, the question is not merely whether the unaided efforts of nature may not, after an

indefinite length of time, accomplish the delivery ; but whether the process will be completed before the constitution of the patient shall have suffered more than the ordinary shock. A patient may be delivered naturally, and yet die of the labour.

In some cases there can be no doubt ; the deficiency of uterine power, or the existence of constitutional disturbance is so marked, that prompt assistance of some kind is imperatively demanded. But in another class these indications are less plainly marked ; the pains cannot be said to be altogether inefficient ; they might even complete the labour if sufficient time could be allowed ; but the patient is beginning to suffer constitutionally, or locally, from the prolongation of labour ; and there are evidences, that more danger may result from delay than from interference. Now, in these cases, where the power is inefficient, or altogether deficient, we have good ground for interference, provided we possess instruments which neither injure mother nor child, and which from their mechanical arrangements are calculated to assist feeble pains, or to supply their absence.

For those cases where the deviation is dependent upon physical disproportion, it may be necessary that one life should be sacrificed to secure the other ; and all that can be required is, that the instruments used, be accurately adapted to secure the latter as well as to effect the former.

Thus, Sir, whether we examine the cases we meet with, and decide upon the instruments required, or whether we classify the instruments we possess, we shall find them naturally divided into—

1st. Those which do not injure either mother or child, as the vectis and forceps ; and

2nd. Those which are employed in the destruction and extraction of the child, but which are not intended to injure the mother, as the perforator and crotchet, and the cephalotribe.

Each of these instruments I propose to investigate in turn ; and as my first contribution to the Obstetrical Society, this ses-

sion, I beg permission to offer you, Sir, the results of my researches on the subject of the *Vectis*, or *Lever*.

I confess that so many claims have been put forth to the invention of this simple instrument, that it is not very easy to trace it to its author. It has been ascribed to Celsus, to Mauriceau, to Schitling, and to Palfyn; but the credit, so far as I can judge, belongs either to Henry Roonhuysen, or Chamberlayne.^a That Roonhuysen possessed the secret there can be no question; and among the instruments belonging to the Chamberlaynes, discovered at Woodham Mortimer Hall, West Malden, Essex, the vectis was found.^b Mr. Cansardine, who published the account of them in vol. ix. of the *Medico-Chirurgical Transactions*, says:—

“ With respect to the instruments I would briefly observe, that they appear to me to contain within themselves the most decided and conclusive evidence of originality of invention; and that even the progress of this invention may be distinctly traced

^a Herbineaux, *Accouch. Laborieux*, vol. i. p. 17.

^b In Dr. Hugh Chamberlayne's Address to the Reader, prefixed to his Translation of Mauriceau, 1716, after stating the custom of using hooks to bring away the child in difficult labours, he observes, “ But I can neither approve of that practice, nor of those delays, (beyond twenty-four hours,) because my father, brothers, and myself (though none else in Europe as I know) have, by God's blessing, and our industry, attained to, and long practised a way to deliver women in this case, without any prejudice to them or their infants; though all others (being obliged, for want of such an expedient, to use the common way) do and must endanger, if not destroy, one or both with hooks. By this manual operation a labour may be despatched (in the least difficulty) with fewer pains and sooner, to the great advantage, and without danger, both of woman and child. If, therefore, the use of hooks by physicians and chirurgeons be condemned, (without thereto necessitated through some monstrous birth,) we can much less approve of a midwife's using them, as some here in England boast they do, which rash presumption in France would call them in question for their lives.”

I am not quite sure that a good argument in favour of Roonhuysen's being the discoverer, might not be drawn from the fact, that although it was known in England that the Dutch professed some marvellous means of delivering women, yet a knowledge of the use of the vectis was not communicated, as far as we know, to any one by the Chamberlaynes, whilst the discovery of the forceps certainly was.

in its different stages through the mind of the inventor. First we have a simple vectis with an open fenestra, supposed to be of much more recent invention." It is known that many years after Dr. Chamberlayne's visit to Paris, about the year 1693, he paid a visit to Holland, and that he there became acquainted with Roonhuysen; but whether he communicated a knowledge of the vectis to Roonhuysen, or Roonhuysen to him, does not seem clear.—(*Ramsbotham*.) The majority of writers (foreigners especially) take the latter view, but some maintain the former; I cannot undertake to decide the point.

It is clear, however, that each possessed the instrument and used it, and in one respect they agreed. They kept it secret, except to a chosen few, to whom it was communicated for large sums of money. Roonhuysen appears to have first taught his method to his son Roger, to Professor Ruysch and Corneille Boeckmann, and the three, on 21st March, 1709, solemnly agreed to instruct (for a consideration) Jean de Bruyn and Pierre Plattmann, providing they observed the rule of secrecy. De Bruyn practised with the vectis forty-two years, and stated that he had delivered 800 children by it. He died in January 1753, having previously communicated the secret to Reinier Boom, who in his turn instructed Paul de Wind and his brother Gerard de Wind in its use. Plattmann meantime had taught François Rooy, and Boeckmann, it is said, Monsieur de Moor.

Up to this period the instrument was known to nine or ten persons, but the secret was faithfully kept. The individuals gained a great profit by its possession, but the science of midwifery was not advanced. At this time (1753, *Camper*.) two Dutch practitioners, MM. Jacques de Vischer and Hugo Van du Pol, whose names deserve most honourable mention, and more especially as they did not practise midwifery, (*Herbinaux*), conceived the project of making public a discovery which promised such valuable results. They bought the secret for a large sum of money (Baudelocque says 5000 livres de France) of

Gertrude de Bruyn, daughter of Jean de Bruyn and wife of Herman Van der Heiden, and immediately published an account of it in the Dutch language, thus terminating the secret history of the vectis.^a I have not seen the essay, but from an abstract of it translated into French by M. de Preville and affixed to his translation of Smellie's *Midwifery*, many of these facts have been taken.

I have not been able to ascertain that the Chamberlaynes imparted a knowledge of the vectis to any practitioner in this country, although at the time of the publication of Vischer and Van der Pol the forceps was ordinarily used in London. Dr. Denman remarks : " When the vectis was very much used and highly esteemed at Amsterdam, as an invaluable improvement in the practice of midwifery, the forceps was the favourite instrument in this country, especially as lastly altered by Smellie, who was then the principal teacher of the art in London. But the chief practice in this city was successively in the hands of Drs. BAMBER, GRIFFITH, MIDDLETON, NESBIT and COLE, some, if not all, of whom, except Dr. Bamber, whose forceps I have seen, preferred the vectis to the forceps. To these gentlemen succeeded Dr. John Wathen, a man of great ingenuity and most pleasing manners, who altered the form and reduced the size of the vectis, and frequently used it with a dexterity that has astonished me. In the year 1757 that most excellent charity for delivering poor women at their own habitations was established ; and Dr. John Ford was the first physician appointed to conduct it. On every occasion which required instruments

^a Dr. Breen in his paper on this subject observes : " Van der Pol settled at Canterbury in England, and assumed the name of Dawkins, but probably from his ignorance of the English language did little to spread the knowledge of the mode of using the obstetric lever."

There are several discrepancies between the account I have given, and that found in different authors, though not to any very great amount.—I have given references to the works on the authority of which my statements are made, and as far as possible I have referred to originals.

of this kind Dr. Ford used the vectis; and his coadjutors and successors, Drs. Cooper, Cogan, Douglass, Sims, Dennison, Squire and Croft, with many others, have followed his example. From the deserved reputation of these gentlemen, who have at all times expressed their approbation of the vectis in preference to the forceps, many have been induced to try it, and the general opinion of its utility has increased. At the present time, all who are engaged in the practice of midwifery would consider themselves as deficient, if they were not acquainted with the structure and manner of using the vectis; some who formerly preferred and used the forceps have relinquished the use of this instrument for the vectis; and others who, from education or habit, continue to use the forceps, are very willing to allow the equal if not superior utility and convenience of the vectis.”^a

It is evident that the excess of praise bestowed upon the lever has been injurious, and by exciting undue expectations has led to disappointment, and, as a consequence, to the opposite extreme of depreciation.

Amongst English writers, I find it unnoticed by Exton (1751), Chapman (1753), Pugh (1754), Cooper (1766), Johnson (1769), Memis (1765), Burton (1769), Foster (1781), and several others about the same date.

Mr. Dease of this city, whose work is dated 1783, observes of the lever of Roonhuysen: “the instrument was applied to the occiput of the child and against the symphysis of the pubis, by elevating the handle towards the belly of the woman, the forehead of the child was pressed down against the sacrum instead of being raised from it, and the perineum must inevitably in the greater number of cases have been miserably lacerated.

“On considering these circumstances I was inclined to prefer and improve the lever of Roonhuysen; the form which I have given it, from repeated experience, I find answers the in-

^a Introduction to Midwifery, 7th Ed. p. 284.

tention of the forceps, without being liable to the many disadvantages we meet with in using the latter."^a

Dr. Osborn (1792) institutes a comparison between the forceps and lever, in which he (and justly, I think) gives the preference to the former. On the latter he makes the following remarks: "It seems extremely probable that the vectis or simple lever was employed in laborious or difficult labours before the more complicated lever or forceps was had recourse to in such cases; for, comparing the situation of the child's head in the cavity of the pelvis, with difficulties of a similar nature which must have occurred to every man's observation, such as the removing impediments of great weight with the common lever, it was hardly possible not to apply such observations to the exactly similar situation of the child in the living woman, and to endeavour to effect that relief, by those very means, which were known to be effectual on inanimate matter."^b Dr. Hamilton, sen. of Edinburgh (1784) observes: "The secret of the celebrated Roonhuysen is extremely limited in its uses."^c Dr. Bland (1794) takes the other side of the question, and in opposition to Osborn, defends the vectis, and gives it the preference over the forceps.^d

In the *Edinburgh Practice of Midwifery* (1803) we find large quotations from Dr. Denman, whose opinion it would seem was conclusive with the author of that compilation.

Dr. J. Clarke, London, (1808), describes but evidently dislikes the vectis; he concludes: "So that we either cannot use much force with the vectis, or if we do, it will be at the certainty of doing much mischief."^e Dr. Power (1819) omits all notice of it. Dr. Burns of Glasgow takes a very fair view of its use and value. He says: "We shall always find the lever

^a *Observations in Midwifery*, pp. 42, 46.

^b *Essays on the Practice of Midwifery*, p. 116.

^c *Outlines of the Theory and Practice of Midwifery*, p. 267.

^d *On Human and Comparative Parturition*, p. 180.

^e *London Practice of Midwifery*, 6th Ed. p. 209.

more or less effectual, in proportion to the assistance afforded by the uterus itself, and it ought not to be employed, when we have no reason to expect the active co-operation of the pains. It should be considered more in the light of an aid to the pains than the forceps, and more dependent on them for success, consequently more limited in its utility. In this view it is a subordinate instrument, in so far as it is used in milder cases of arrest, which perhaps might ultimately have terminated by the natural efforts, but to which, it might not have been prudent longer to have trusted. The pains may not be strong, but still they assist the instrument, and are generally excited by it to greater efficacy, otherwise we do less good.”^a

Dr. James Hamilton, jun., Prof. of Midwifery in Edinburgh, (1826,) remarks: “The other instrument consists of a single blade called the vectis or lever, calculated to press on the head of the infant and to increase the efficacy of the labour pains; but as it is quite inefficacious when the uterine contractions are suspended, and as in the hands of an inexperienced practitioner it may do considerable injury, both to the infant and the parent, it should only be employed in certain cases, where there is a slight degree of narrowness at the brim of the pelvis, or where the face of the infant is forced forward.”^b

Dr. Gooch attributes little or no value to the vectis on account of its only aiding feeble pains, and not supplying their place.^c Dr. Conquest draws a pretty accurate comparison between the forceps and vectis. “Some persons have lavished the highest praise on the one instrument, and equally eminent men have bestowed the most unqualified approbation on the other. As in most disputed points, ‘*Media quodammodo inter diversas sententias*’ will hold good here; for whilst, under some circumstances, the lever is doubtless preferable to the forceps, the latter is now very generally admitted to

^a Principles of Midwifery, 9th Ed. (1837) p. 495.

^b Outlines of Midwifery, p. 52.

^c Lectures by Skinner, p. 215.

be, in the majority of cases, by far the most useful instrument."^a

Dr. Ashwell (1834) attaches but little comparative value to it.

Dr. Maunsell observes, "that either instrument (forceps or vectis) may be employed by competent persons with perfect safety."^b

In his valuable work on the Principles and Practice of Obstetrics (1834) Dr. Blundell observes: "The next instrument, the use of which I shall mention, is the tractor or lever, an instrument excellent and of great effect in dexterous hands. If skill and judgment are wanting, even the tractor (vectis) may inflict dreadful injuries; but in such hands, still greater mischief may be expected from the long forceps; to you therefore I recommend its use as the safer instrument of the two, possessing as it does in an eminent manner the advantages of portability and ready application."^c

Dr. F. Ramsbotham (1834) expresses his opinion of the utility of the vectis when used as a tractor.^d

In the year 1835 a distinguished practitioner of this city, Dr. Breen, one of your Vice-Presidents, published an able essay in defence of the vectis in the Dublin Journal.^e

Having entered with sufficient minuteness into the history of the vectis in Great Britain, let us turn for a few moments to the literature of the continent. In France, Mauriceau, before the vectis had been publicly described, invented something similar for the purpose of extracting the head when separated from the body.

"In 1715 Isaac de Bruas of Middleburg made an attempt to extract a child when the head presented, with a blunt hook, such as he had been accustomed to use in breech presentations. He

^a Outlines of Midwifery, p. 103, 5th Ed.

^b Dublin Practice of Midwifery, p. 134.

^c p. 509.

^d Lectures on Midwifery, in Med. Gazette, May 31, 1834. ^e Vol. vii. p. 353.

succeeded in his object, having slightly bruised the child's head. Correctly judging that the instrument was too thick, and not sufficiently wide for this purpose, he formed a fenestrated vectis, decidedly the best of all those first invented, and which indeed approaches nearer than any of them to the form of Lowder."^a At a later period, that is "in 1738, Rigaudeaux being called to a case in which the head was impacted, procured a common chemist's spatula, a foot in length, and after having softened the blade in the fire, he bent into a slight curve, and with it delivered the woman of a live child. Incited by this success he formed an instrument very similar to Titsing's in shape, though shorter, which he was in the habit of using continually. Warroquier of Lisle also, it would seem, fell by chance upon the expedient of delivering by the lever, before that instrument was publicly known: for in 1753 being foiled in his attempt to terminate a labour by Smellie's forceps, at that time but just come into use, he employed one blade as a vectis, and had the satisfaction of bringing into the world a living infant. From that time he discarded the double instrument, and used a single blade not unlike Titsing's in fashion; with which indeed he afterwards boasted that in twenty-one years he had delivered 1200 women."^b Long before this time however, Palfym seems to have had some idea of the instrument, if we may judge by the plate given on his authority by Heister; and as the result of his inquiries he presented to the French Academy of Sciences in 1720 his "tire tête."

But though these attempts were made to supply the want of the vectis, I do not find that there was any regular application of such an instrument before the publication of Vischer and Van der Pol. No writer before Levret, that I possess, alludes to it. To Levret, however, it appears to have been fami-

^a Ramsbotham's Lectures, Med. Gaz. May 31, 1834, p. 307.

^b Ibid. and also Baudelocque l'Art des Acc. vol. ii. p. 98. Camper's Memoirs. Mem. de l'Acad. de Chir. vol. xv. p. 482.

liar. He places it lower than the forceps in point of utility, but recommends it when the head is on the point of being "enclavée."

About 1785 M. Herbiniaux of Brussels published a treatise on laborious labours, in which he appears as the advocate of the vectis.^a This was criticized by MM. Alphonse Le Roy and Baudelocque, and in 1791 a new edition appeared with comments on those criticisms. It must be confessed that he does not spare his opponents, but neither do M. Baudelocque's remarks exhibit much of the milk of human kindness. The latter author concludes:—"Ce n'est pas contre l'utilité du levier mais contre l'abus qu'on en a fait, que nous nous sommes élevés: notre intention dans toutes les discussions où nous sommes entrés, n'a pas été de le proscrire, mais de le faire voir qu'on l'avoit employé sans principes, et presque toujours en des circonstances où l'on pouvait s'en passer, où le doigt méthodiquement dirigé pouvoit suffire, où les forces même de la nature n'avoient besoin d'aucun aide."^b

M. Maygrier (1814) admits a certain though slight amount of utility; he says: "Cet instrument, dont Roonhuysen est l'inventeur, a été beaucoup trop vanté, et comme tous les instruments nouveaux, beaucoup trop employé. Aujourd'hui, réduit à sa juste valeur, il ne sert que dans quelques cas rares, où la tête, se plaçant défavorablement au détroit supérieur, n'a besoin que d'un léger mouvement, pour pouvoir s'engager convenablement."

M. Deleurye^d and M. Hoin took part with Levret, and M. Chayrou opposed them.

M. Goubelly (1778) is an advocate of the vectis.

Mad. Boivin (1817) has a very low opinion of this instru-

^a Vol. i. p. 17.

^b L'Art des Accouch. vol. ii. p. 98.

^c Nouv. Elem. de l'Accouch. p. 406.

^d Traité d'Accouch. p. 247.

ment, for she observes: "Le levier, cet instrument si long temps mysterieux chez les Hollandais, le levier lui-même, commence à tomber en desuetude malgré son apologiste Herbiniaux."^a

And Mad. La Chappelle throws it overboard altogether: "Le levier, qui a fait tant de bruit entre les mains de Roonhuysen, est tombé maintenant dans une telle défaveur, que je crois inutile d'ajouter aux critiques qu'on en a faites."^b

M. Capuron admits that it may serve to correct a malposition of the head: "L'unique but qu'on se propose en France avec le levier, est d'agir sur l'occiput pour l'abaisser, et pour faire remonter en meme temps le menton vers la poitrine."^c In this opinion M. Gardien^d and M. Murat^e concur. M. Velpeau, however, seems to have formed a more correct judgment of the value and the use of the vectis.^f

As it was amongst the Dutch the vectis originated, so do they appear to have estimated it most highly and cultivated it most successfully.

In addition to the names of Henry and Roger Roonhuysen, I have already mentioned those of Ruysch, Boekelmann, De Bruyn, Plattmann, Boom, Rooy, De Moor, Vischer, and Van der Pol; I may now add Titsingh, Palfyn, Berkmann, Van der Haar, Stylecke, Jans, De Bree, De Bruas, Van Geuns, Rathlaaw, &c. Van Sweeten, in his Commentaries upon the Aphorisms of Boerhaave,^g published in 1754, refers to the discovery of this instrument as a benefit conferred on the human race. He remarks: "Quamvis autem egregii viri, qui varios forcipes invenerunt, aut perfecerunt, omnem laudem mereantur, et ob

^a Memorial, p. 273.

^b Prat. des Accouch. p. 60.

^c Principes de l'Art des Accouch. p. 449.

^d Traité d'Accouch. vol. ii. p. 495, 1824.

^e Dict. de Med. Art. Levier, vol. xxviii.

^f De l'Art des Accouch. p. 428, Brussels Ed.

^g Vol. ix. p. 253 of the Ed. of 1790, in 12 vols.

industriam et ob candorem, quo sua inventa publicò communi-
caverunt, tamen videtur *vectis* ille *Roonhuysianus* reliquis
esse præferendus." The celebrated Camper published a pa-
per in 1774, in which he advocated the use of the lever, and
spoke highly of its advantages.^a In 1794 Johannes Mulder
published a very learned and valuable treatise on the forceps
and *vectis*,^b with measurements and plates of which I shall
avail myself in the course of this paper.

I have already enumerated M. Herbiniaux amongst French
writers, though he rather belongs to the present section, as he
was a practitioner at Brussels. It is also described by J. J.
Plenck^c (1781), Boer^d (1785), Gehler^e (1789), Hoffmann^f
(1766), Rechberger^g (1779), Wolff^h (1774), Zellerⁱ (1789),
L. F. Froriep^j (1832), E. Rosshirt^k (1835), H. F. Kilian^l
(1834).

Having thus given the history of the lever as perfectly as
my access to books will permit me, I shall next direct your
attention to the instrument itself. I shall give the description
of M. Roonhuysen's lever first, and then noticing one or two
variations, describe the one commonly in use in this country,
and give the tables and plates of M. Mulder.

" L'instrument de Roonhuisen est un morceau long et
quarrè de fer bien forgé, de $10\frac{3}{4}$ pouces de long et large d'un
pouce : son epaisseur sans être garni est de $\frac{1}{8}$ d'un pouce, et

^a Mem. de l'Acad. de Chir. vol. xv. p. 246. Herbiniaux, vol. i. p. 57.

^b Historia Literaria et Critica Forcipum et Vectium Obstetriciorum.

^c Elementa Artis Obstetriciæ, p. 187.

^d Abhandlung von dem Gebrauche, &c. des Hebels.

^e Prog. de Vectis obstet. Usu dubio.

^f De Forcipe Smellii anteponenda Vecti Roonhuysiano.

^g Bekanntmachung einer besonderen art von Hebel, &c.

^h De Vecti Roonhuysiano emendato.

ⁱ Bemerkungen über einige Gegenstände, &c.

^j Handbuch der Geburtshülfe, p. 462.

^k De Anzeigen zu dem Geburtshülf. Operation, p. 137.

^l Die Operative Geburtshülfe, vol. ii. p. 686.

étant garni, de $\frac{5}{8}$ d'un pouce. Ce fer est droit au milieu de la longueur de 4 pouces, et se courbe insensiblement vers les extrémités. Ces combures sont à peu près semblables, et étant mesurées dans leur concavités, elles ont 3 pouces $\frac{1}{4}$ de courbure et environ $\frac{5}{8}$ de pouce de fond. Ce levier de fer doit être soigneusement arrondi des tous cotès, et principalement aux quatre coins, afin qu'il ne puisse pas faire du mal lorsqu'on l'appuie. C'est pourquoi les extrémités des combures quoique bien arrondis, doivent être garnies d'un emplâtre de diapalme étendu sur du gros linge de la longueur d'un pouce en dedans ; le morceau droit du milieu situé entre les deux combures, et par lequel se fait la plus forte pression contre les os pubis, doit être tout à fait garni de cet emplâtre, et un peu plus forte au milieu. Il faut surtout avoir attention que ces emplâtres soit appliqués fort également sur le fer, sans le moindre pli. Après avoir garni le fer de ces emplâtres, on le garni tout entier de peau de chien mince et fort douce, et il faut observer que cette peau doit être appliquée fort unie, et que les coutures de la peau soient au dehors c'est à dire du côté convexe de l'instrument." It is added, " Nous avons trouvé une petite corde entortillée autour d'un des bouts de l'instrument, dans l'endroit où la courbure est plus grande, comme on le voit même dans la figure ; ce que nous croyons ne servir à autre chose, si non pour marquer qu'on doit se servir de ce côté plutôt que l'autre, ou pour mesurer l'approche de l'instrument."^a

" I have said that Roonhuysen's lever consisted of a flat piece of iron bent into a slight curve at both ends, and he generally employed it covered with soft leather. Titsing fancied he had improved on this plan by padding the instrument with wool. It has been formed by different persons—either for the sake of appearance, or from the presumption that such substances were less likely to inflict injury than the harder metal—

^a Memoir translated from the Dutch, and affixed to M. de Preville's translation of Smellie, vol. iv. p. 2.

of wood, horn, ivory, and silver. The vectis of Morand in 1755 was of ivory, that of Herbiniaux in 1782 of the latter material."^a

"When the vectis was first known in this country, that described by Heister was preferred to those recommended by the surgeons of Amsterdam. The vectis used by Dr. Cole was like one blade of the forceps, somewhat lengthened and enlarged. That of Dr. Griffith was of the same kind, with a hinge between the handle and the blade; and that of Dr. Wathen was not unlike Palfyn's, but with a flat handle and a hook at the extremity of the handle, which prevented its slipping through the hand, and might be occasionally used as a crotchet. Many other changes have been made in the construction of the instrument, but the vectis now generally used is of the following dimensions the whole length of the instrument before it is curved is $12\frac{1}{2}$ inches. The length of the blade before it is curved is $7\frac{1}{2}$ inches. The length of the blade when curved is $6\frac{1}{2}$ inches. The widest part of the blade is $1\frac{3}{4}$ inches. The weight of the vectis is $6\frac{1}{2}$ ounces. The handle is fixed in wood."^b Dr. Bland describes one slightly different: "But the properties of the lever will be best seen by giving a cursory description of it. Unlike the forceps, it requires no great nicety in its structure. A flat piece of iron about the breadth of two fingers, from eleven to fourteen inches in length, obtuse, of sufficient strength, perfectly smooth, that it may not abrade or injure the vagina, and lightly curved at one of its extremities, gives the complete idea of a lever capable of doing every thing for which the forceps or lever are usually recommended. Upon the last circumstance, the lightness of its curve, its utility in a great measure depends; when the instrument is much curved, it is introduced with difficulty, and its action upon the head of the child is weak and inconsiderable.

^a Dr. Ramsbotham's Lectures, Med. Gazette, May 31, 1834, p. 306.

^b Denman's Introduction to Midwifery, p. 286.

On the contrary, when the curve is light, just declining from a straight line, it is introduced with the greatest ease, and its power in forcing down the head of the child is very great, and may be used so as to surmount almost every possible difficulty."^a

It is evident that Dr. Bland's lever can only be used as such, and not as a tractor, which is at least a disadvantage, as we shall see hereafter.

Dr. Aitkin of Edinburgh, wishing to avoid the difficulty of introducing a curved instrument, contrived one which he called a living lever: "I have invented one, that by turning a screw, becomes straight, to facilitate its introduction. It resumes a curvature exactly proportioned to the convexity of the part of the child on which it is applied; in consequence its pressure is more diffused and less injurious. I have taken the liberty to call it **LIVING LEVER**, because its motion resembles that of the finger."^b

Dr. D. Davis has given a representation of a lever armed with small teeth, to be used in changing the position of the head.^c

The one in ordinary use is that described by Dr. Lowder, and improved by Mr. Gaitskill, who says: "The vectis should be thirteen inches in length, one half to form the handle, the other the curve. The handle should be made of hard wood, rendered rough for the purpose of obtaining a firmer hold, and made to screw on and off. When the instrument is made with a hinge handle, it is very difficult to introduce: therefore this construction of the instrument should never be adopted."^d

Baudelocque, speaking of the French practice, observes: "Le levier que les Français ont substitué à celui-ci, ressemble assez bien à l'une des branches du forceps de Palfyn, si ce

^a Human and Comparative Parturition, p. 187.

^b Principles of Midwifery, p. 73.

^c Elements of Operative Midwifery.

^d London Med. Repository, Nov. 1823, pp. 379-80-81. Blundell, p. 509.

n'est qu'il est plus étroit et plus allongé, et que sa combure est bordé intérieurement d'un filet, semblable à celui qui règne autour les cuillers du forceps courbe de Levret. Pour le rendre plus utile, il faudroit le courber d'avantage, et lui donner la moitié de sa largeur de plus, comme quelques uns l'ont déjà proposé et fait exécuter.”^a

The following measurements are extracted from Mulder's History of the Vectis.

VECTIUM AUCTORES.	LAMINÆ.							
	Longitudo.						Altitudo.	
	Curvaturæ.			Curvaturæ.			Curvaturæ Majoris.	Curvaturæ Minoris.
	Vectis.	Majoris.	Minoris.	Majoris.	Minoris.	Partis Intermediæ.	Supra Lineam Horizont.	Infra Lineam Horizont.
De Bruyn 1, . . .	10 $\frac{3}{4}$ p.	4 p.	2 $\frac{3}{4}$ p.	1 p.	1 p.	1 p.	$\frac{5}{12}$ p.	$\frac{1}{2}$ p.
Do. 2, . . .	10 $\frac{3}{4}$ p.	3 $\frac{1}{2}$ p.	3 $\frac{1}{4}$ p.	1 p.	1 p.	1 p.	$\frac{2}{3}$ p.	$\frac{7}{12}$ p.
Boom,	10 $\frac{3}{4}$ p.	5 p.	4 $\frac{1}{4}$ p.	1 p.	1 p.	1 p.	$\frac{1}{2}$ p.	$\frac{1}{4}$ p.
Morand,	11 p.	5 p.	4 $\frac{3}{4}$ p.	1 $\frac{1}{8}$ p.	1 $\frac{1}{8}$ p.	1 $\frac{1}{8}$ p.	$\frac{2}{3}$ p.	$\frac{2}{3}$ p.
Fleurant,	11 p.	8 p.	7 $\frac{1}{2}$ p.	1 $\frac{1}{8}$ p.	1 $\frac{1}{8}$ p.	1 $\frac{1}{8}$ p.	$\frac{5}{6}$ p.	$\frac{5}{6}$ p.
Camper 1, . . .	10 $\frac{1}{8}$ p.	4 $\frac{3}{4}$ p.	4 p.	1 $\frac{5}{16}$ p.	1 $\frac{5}{16}$ p.	1 $\frac{5}{16}$ p.	$\frac{7}{12}$ p.	$\frac{1}{2}$ p.
Do. 2, . . .	11 p.	5 $\frac{1}{4}$ p.	5 p.	1 p.	1 p.	1 p.	1 p.	1 p.
Do. 3, . . .	10 $\frac{3}{4}$ p.	5 $\frac{1}{6}$ p.	4 $\frac{3}{4}$ p.	1 $\frac{1}{8}$ p.	1 $\frac{1}{8}$ p.	1 $\frac{1}{8}$ p.	1 $\frac{1}{6}$ p.	1 $\frac{1}{6}$ p.
Do. 4, . . .	10 $\frac{3}{4}$ p.	5 p.	4 $\frac{3}{4}$ p.	1 p.	1 p.	1 p.	$\frac{5}{6}$ p.	$\frac{5}{6}$ p.
Do. 5, . . .	11 $\frac{5}{12}$ p.	5 $\frac{3}{4}$ p.	4 $\frac{1}{2}$ p.	1 $\frac{1}{8}$ p.	1 $\frac{1}{8}$ p.	1 $\frac{1}{8}$ p.	1 $\frac{1}{4}$ p.	1 $\frac{1}{12}$ p.
Do. 6, . . .	10 $\frac{3}{4}$ p.	4 p.	3 $\frac{1}{4}$ p.	1 $\frac{1}{16}$ p.	1 $\frac{1}{16}$ p.	1 $\frac{1}{16}$ p.	$\frac{5}{12}$ p.	$\frac{1}{3}$ p.
Rechberger, . .	11 p.	3 $\frac{3}{4}$ p.	3 $\frac{1}{2}$ p.	1 $\frac{1}{2}$ p.	1 $\frac{1}{2}$ p.	1 $\frac{1}{6}$ p.	$\frac{5}{12}$ p.	$\frac{1}{3}$ p.
Van Wy. 1, . . .	10 $\frac{3}{4}$ p.	5 p.	4 $\frac{2}{3}$ p.	1 $\frac{1}{12}$ p.	$\frac{3}{4}$ p.	$\frac{3}{4}$ p.	$\frac{1}{12}$ p.	$\frac{2}{6}$ p.
Do. 2, . . .	13 p.	5 $\frac{3}{4}$ p.	5 $\frac{1}{2}$ p.	1 $\frac{1}{2}$ p.	1 p.	1 p.	1 $\frac{1}{6}$ p.	1 $\frac{1}{6}$ p.
Sleurs 1,	11 $\frac{1}{2}$ p.	5 p.	4 $\frac{2}{3}$ p.	1 p.	1 $\frac{1}{6}$ p.	$\frac{2}{3}$ p.	1 p.	1 p.
Starke,	11 p.	5 $\frac{2}{3}$ p.	5 $\frac{1}{3}$ p.	1 $\frac{1}{6}$ p.	1 $\frac{1}{12}$ p.	1 $\frac{1}{3}$ p.	$\frac{2}{3}$ p.	$\frac{5}{12}$ p.
Zeller,	11 $\frac{2}{3}$ p.	4 $\frac{2}{3}$ p.	4 $\frac{1}{3}$ p.	2 p.	1 $\frac{2}{3}$ p.	1 p.	$\frac{1}{3}$ p.	$\frac{1}{4}$ p.
Bland,	13 $\frac{1}{4}$ p.	5 $\frac{1}{2}$ p.	2 $\frac{3}{4}$ p.	1 $\frac{1}{2}$ p.	1 $\frac{1}{3}$ p.	1 p.		

^a De l'Art des Accouch. vol. ii. p. 31, 6th Ed. 1822.

VECTIUM AUCTORES.	SPATULÆ.						
	Longitudo.			Latitudo.		Altitudo.	
						Curvaturæ.	
	Vectis.	Curvaturæ.	Fenestræ.	Maxima Curvaturæ.	Fenestræ.	Supra Lineam Horizont.	Infra Lineam Horizont.
Titsing,	10 $\frac{1}{2}$ p.	5 $\frac{3}{4}$ p.	..	1 p.	..	$\frac{3}{4}$ p.	..
Rigaudeau,	8 $\frac{1}{2}$ p.	5 $\frac{1}{3}$ p.	..	1 p.	..	$\frac{3}{4}$ p.	..
De Bruas,	12 p.	6 $\frac{1}{4}$ p.	..	2 p.	..	$\frac{1}{2}$ p.	1 $\frac{1}{3}$ p.
Camper 7,	12 p.	4 $\frac{5}{6}$ p.	..	1 p.	..	$\frac{2}{3}$ p.	..
Pean,	15 p.	6 $\frac{1}{3}$ p.	3 $\frac{1}{2}$ p.
Goubelly,	13 $\frac{3}{4}$ p.	5 p.	2 $\frac{2}{3}$ p.	1 $\frac{1}{2}$ p.	$\frac{3}{4}$ p.	1 $\frac{1}{3}$ p.	..
Baudelocque,	13 $\frac{3}{4}$ p.	4 $\frac{5}{6}$ p.	2 $\frac{2}{3}$ p.	1 $\frac{1}{2}$ p.	$\frac{3}{4}$ p.	1 $\frac{1}{3}$ p.	..
Herbiniaux,	10 p.	2 $\frac{2}{3}$ p.	2 p.	1 p.	$\frac{1}{2}$ p.	$\frac{1}{2}$ p.	..
Sleurs,	13 $\frac{1}{2}$ p.	4 p.	3 $\frac{1}{2}$ p.	1 p.	$\frac{5}{12}$ p.	$\frac{1}{2}$ p.	7 $\frac{1}{2}$ p.
Lowder 1,	12 $\frac{3}{4}$ p.	7 p.	2 $\frac{3}{4}$ p.	1 $\frac{1}{2}$ p.	1 p.	$\frac{1}{2}$ p.	7 $\frac{1}{2}$ p.
Do. 2,	12 p.	5 p.	1 $\frac{5}{6}$ p.	1 $\frac{3}{4}$ p.	1 $\frac{1}{4}$ p.	$\frac{1}{3}$ p.	1 $\frac{1}{3}$ p.
Do. 3,	11 $\frac{3}{4}$ p.	6 $\frac{1}{4}$ p.	1 $\frac{5}{6}$ p.	1 $\frac{3}{4}$ p.	1 $\frac{1}{4}$ p.	$\frac{1}{2}$ p.	1 $\frac{1}{3}$ p.
Sims,	11 $\frac{1}{4}$ p.	4 $\frac{1}{2}$ p.	2 $\frac{5}{12}$ p.	1 $\frac{5}{12}$ p.	1 p.	..	2 $\frac{1}{6}$ p.
Dennison,	12 $\frac{1}{4}$ p.	3 $\frac{7}{12}$ p.	1 $\frac{1}{4}$ p.	1 $\frac{5}{12}$ p.	1 p.	..	2 $\frac{1}{4}$ p.
Aitken,	13 p.	Varia.	6 p.	1 $\frac{2}{3}$ p.	1 $\frac{1}{6}$ p.	$\frac{1}{2}$ p.	Varia.
De Bree,	11 $\frac{2}{3}$ p.	5 $\frac{1}{2}$ p.	..	1 $\frac{1}{6}$ p.	..	$\frac{3}{4}$ p.	..

The spatulæ differ from the other vectes in having broader ends, and being more curved.

The nature of the aid afforded by the vectis is three-fold :—

1. To correct mal-positions, or aid the natural rotations of the head at the brim, or in the cavity of the pelvis ; and to this the majority of French practitioners limit its employment.

“ It may be employed where a slight stimulus is sufficient to rouse the pains, or where little force is necessary to alter the position of the head, by introducing it in the same manner and with the same precautions as a blade of the forceps, either at the lateral parts of the pelvis, under the arch of the pubis, or diagonally. But there is great hazard of bruising the parts of the mother by the resistance of the instrument, unless managed with so much dexterity that the hand of the operator is the fulcrum or support upon which its action turns.”^a

2. As a lever of the first or second kind, i. e. making a ful-

^a Dr. Hamilton, sen., *Outlines of Midwifery*, p. 201.

Veetis of

1. 2. De Bruyn

3. Boon

4. 5. Tilsing

6. 7. Rigaudau

8. Morand

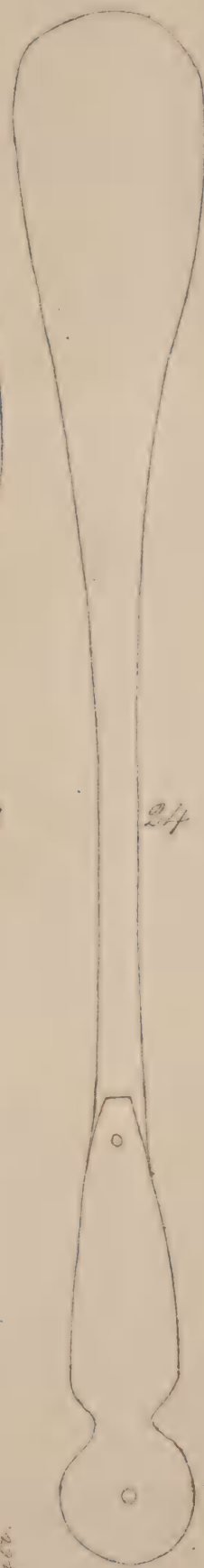
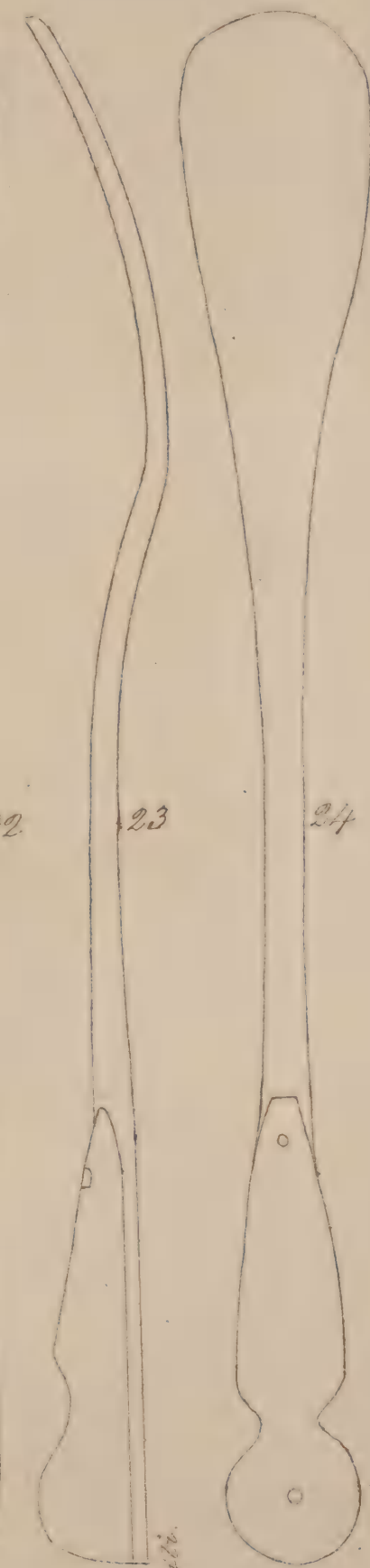
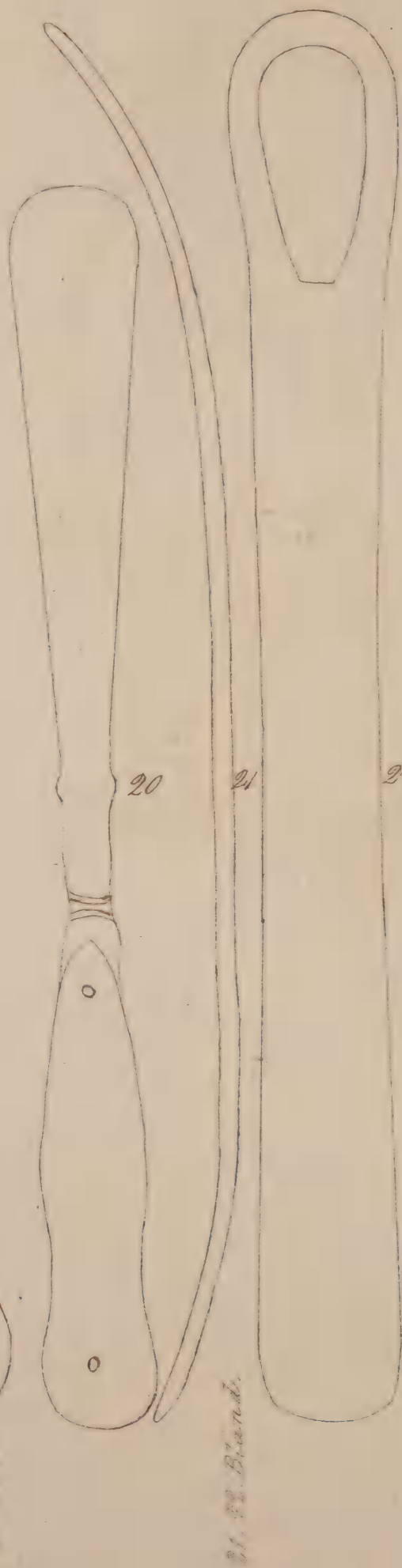
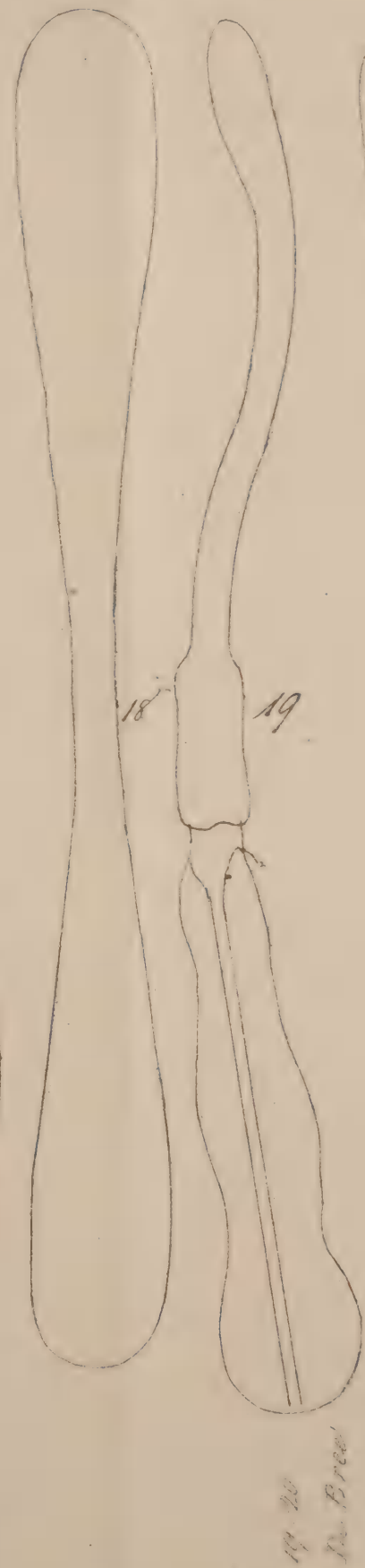
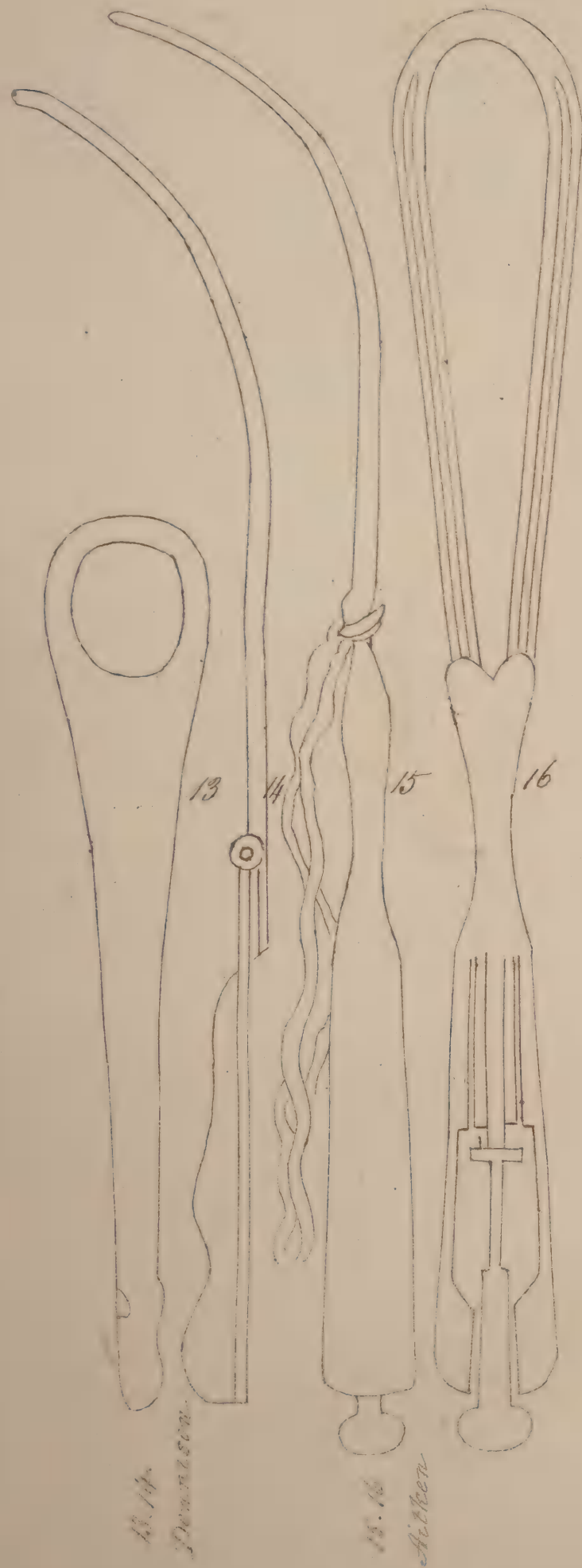
9. 10. 11
de Bruas

12. Fleurant

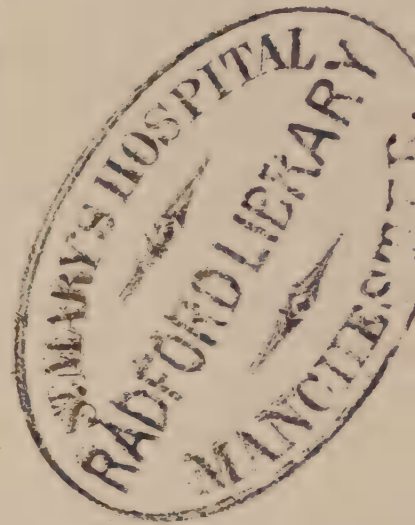
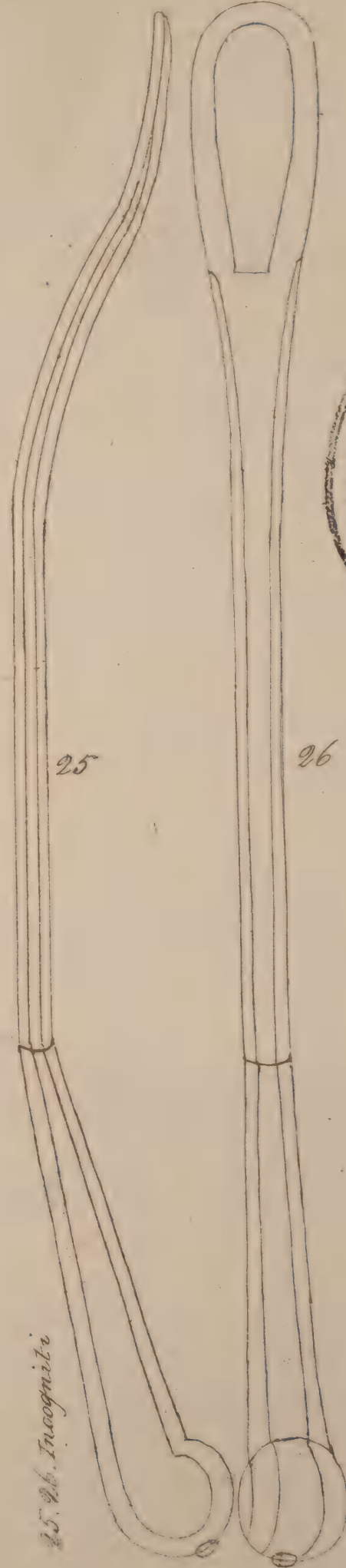
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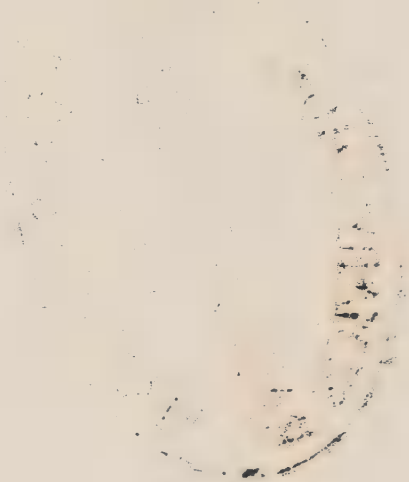
18. 19. 20.
Camper.





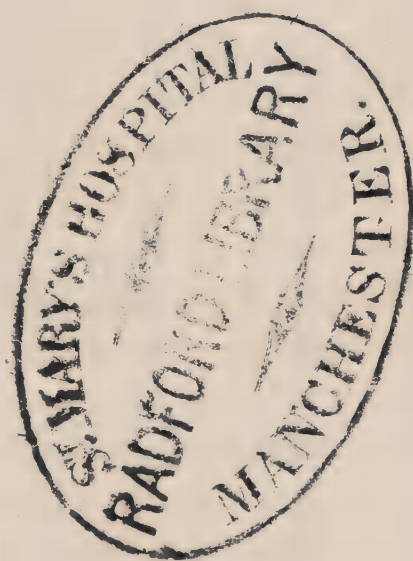
25. 26. *Dr. St. André.*

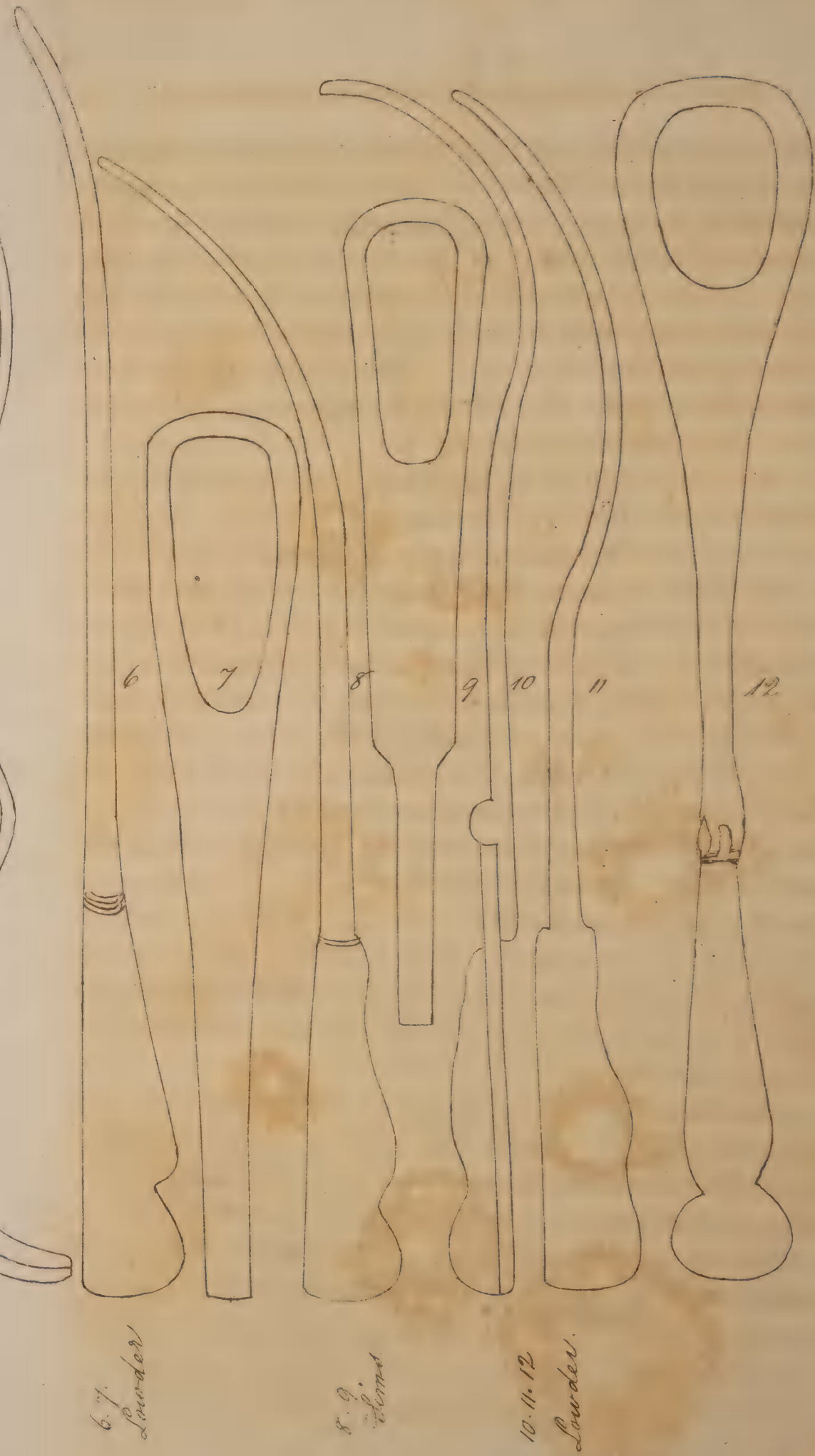
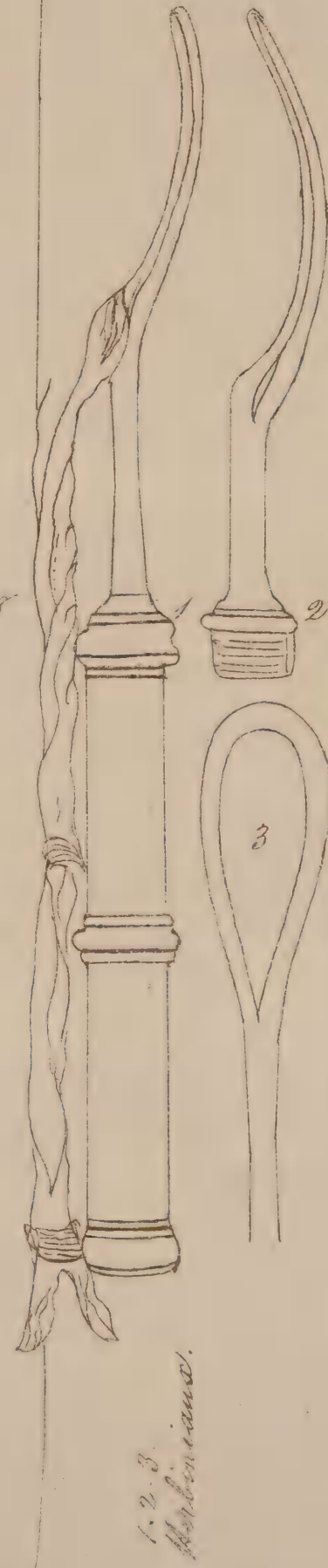
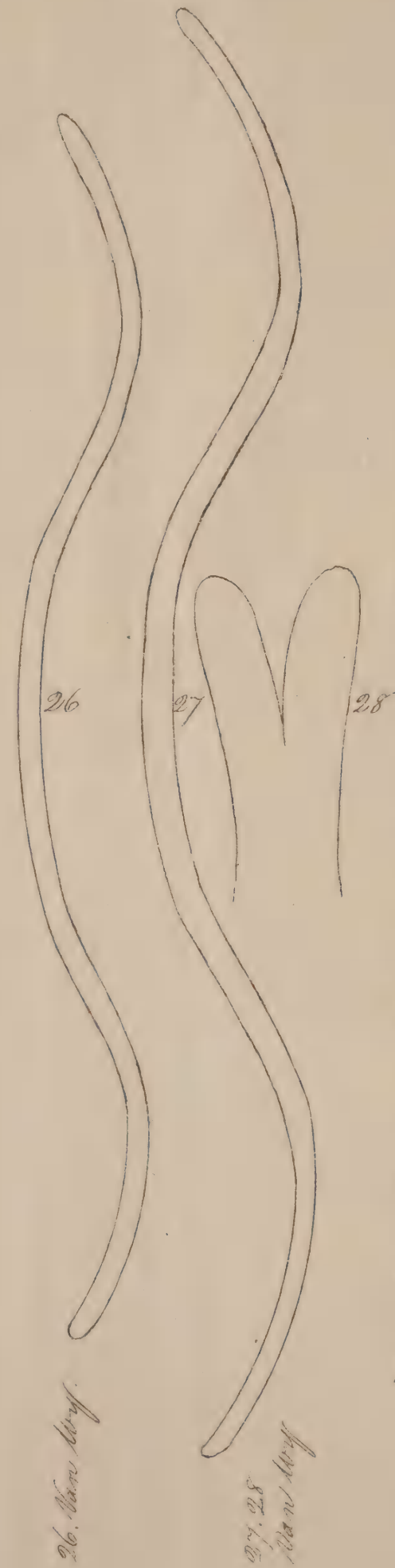
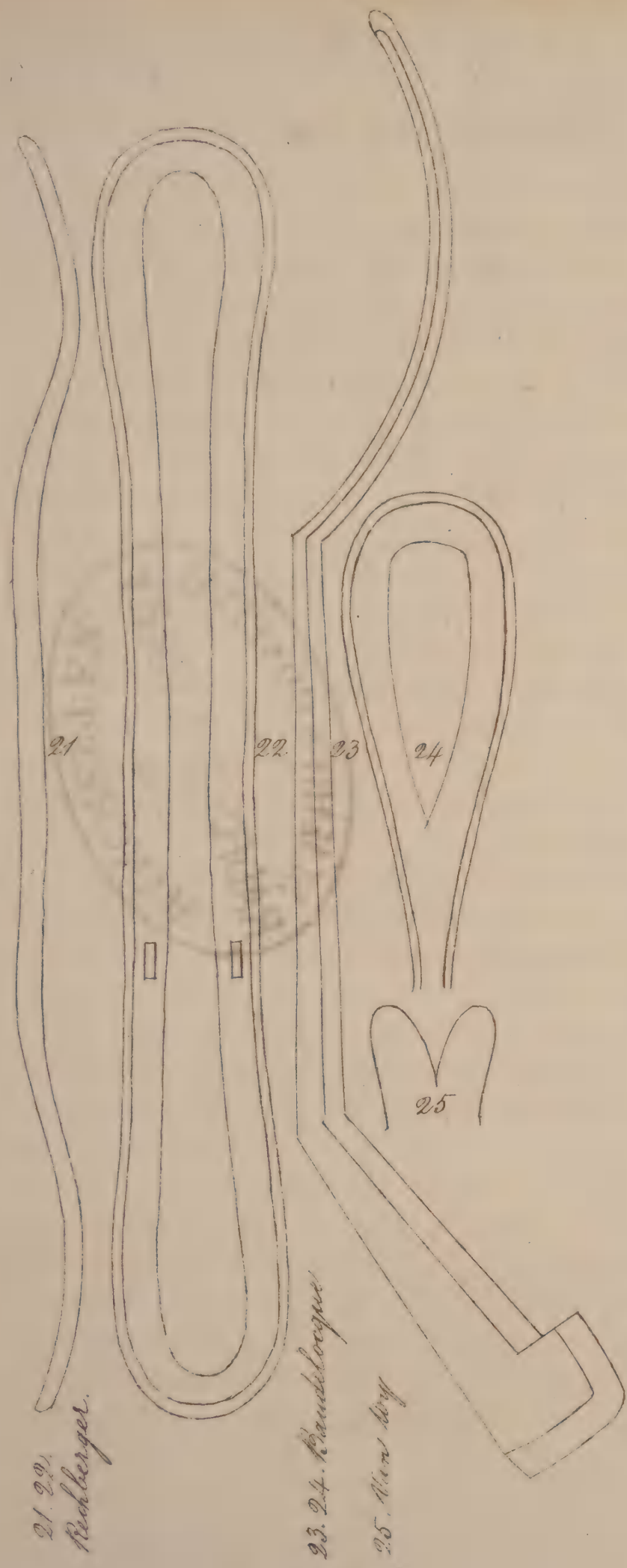




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crum of the pelvis, or of the left hand of the operator external to the pelvis. Its employment in the first way is extremely hazardous, from the certainty of crushing the soft structures lining the pelvis, and the probability of injuring the urethra or the child's head. Many authorities who employ and recommend the lever would altogether reject it, and, I think, justly, rather than so use it. This objection does not hold against the second mode, and this is the proper method if it is to be employed as a lever at all.

“The forceps,” says Dr. Osborne, “is always a lever of the first kind, but the vectis is intended to act, and may be used, either as a lever of the first or of the third kind, according to the manner in which the hands of the operator are employed; for if the right hand be the moving power, and be applied to one end of the instrument, while the other end of the instrument is applied to the child's head, and either the left hand, or any of the bones of the pelvis are made the fulcrum, as they are between the two ends of the instrument, it becomes a lever of the first kind; but if the right hand of the operator is used as a fulcrum at the extremity of the instrument, and the left is applied to the middle of it, or between the two extremities, and is the moving power, it then becomes a lever of the third kind; and thus it is always meant to be employed by M. Herbiniaux, as he himself says, by the additional means of the ligature or string: and so, I believe, it is generally used here, whether with the addition of the string or without it.^a”

The discoverers and first possessors of the secret made the arch of the pubis the fulcrum. In order to avoid the urethra, Boom, Boekelman, and Titsing rested it upon the ramus of the ischium.—(*Mulder*.)

3rd. *As a tractor*.—Dr. Burns says, “it is unfortunately named, for it ought not to be employed to wrench, but to hook or draw down the head; and its proper application would be

^a Essays on the Practice of Midwifery, pp. 83–84.

less apt to be mistaken were it called the tractor."^a This can only be done with the curved vectis; the one used by Roonhuysen could exert no tractile power. When the force thus used is sufficient, it is by far the safest application of the instrument.—(*Herbiniaux. Dease. Ramsbotham. &c.*)

A very high authority remarks, "The instrument then being applied in this manner, you grasp the handle with the right hand, and the middle of the shank with the left; and by the co-operation of the two, pressing down upon the cranium, you support a steady bearing upon the occiput, without, however, resting on any part of the mother as a fulcrum, for the instrument ought to be used not as a lever, but as a tractor."^b

The cases suitable for the employment of the vectis appear to be the following—

1st. Before the head has fully entered the upper outlet, when either from slight malposition, (Froriep,^c) or from very slight narrowing, the uterine efforts are ineffectual in advancing the labour.

"The late Dr. Bromfield, who was thought to excel in skill and address in using the forceps, a few years before his death attended a person in a difficult labour; after waiting the event of the pains until there was reason to fear some great mischief would happen to the woman if he delayed the delivery any longer, as the head of the child was not descended low enough to take hold of it with the forceps with any prospect of success, he began to think of making use of the perforator and crotchet; but first desired the assistance of Dr. Garthshore. Dr. Garthshore, after carefully examining the position of the head of the child, agreed that it would not be proper to apply the forceps, but ventured to assure his colleague, that he had no doubt but that the woman might be delivered by the assistance of the

^a Principles of Midwifery, p. 494.

^b Dr. Blundell. Principles and Practice of Obstetricy, p. 514.

^c Geburtshulfe, p. 464.

lever ; which he accordingly proposed to use. To this proposition Dr. Bromfield at first objected, as he entertained an almost invincible aversion to that instrument ; but thinking it dangerous to delay the delivery longer, and seeing no possibility of bringing the child without opening its head, he consented that the lever should be introduced ; and from the instructions Dr. Garthshore then gave him, he was enabled to deliver the woman with it safely of a living child, in about the space of half an hour.”—(*Bland*.^a)

“ Now the case in which I propose to demonstrate the use of the lever, is that in which I have already been demonstrating the use of the long forceps, and which, among the laborious labours, is of all others the most common in its occurrence ; that labour I mean, in which the cranium is detained at the brim of the pelvis, in consequence of a want of room between the front and the back.”—(*Blundell*.^b)

“ But in another view, it is to be considered as superior, in so far as it may be proposed in cases midway between those admitting the use of the short, and demanding that of the long forceps. Some will say that it can be used whenever the long forceps can be employed.”—(*Burns*.^c)

In these cases it will require a very accurate judgment, lest we use it in cases of too great narrowing, or use it with too great lever power.

Froriep advises it in cases of face presentation,^d and after version when the head is difficult to extract.

2. It was recommended by its early patrons, in cases where the head had become impacted in the pelvis ; in fact it was considered as superseding in a great measure the use of the crotchet. After the description I have given, I need hardly say that it is not merely powerless in such cases, but very likely to be injurious.

^a On Human and Comparative Parturition, p. 192.

^b Princ. and Pract. of Obstetricy, p. 510.

^c Principles of Midwifery, p. 495.

^d Geburtshülfe, p. 463.

“ By the first accounts, it appears that the vectis was recommended, not only in such cases as were thought fit and suitable for the forceps, but to supersede the necessity of our lessening the head of the child. It was in short asserted that no other assistance could in any case be required, beyond that which we were enabled to give with the vectis. But if those accounts were allowed to be true, they would prove the miserable state of the principles and practice of midwifery, at the time and in the country in which they were written, in much stronger terms than they would describe the excellence of the instrument.”

“ The general condition and circumstances of labours before stated as requiring the forceps, will hold good and with equal propriety when the vectis is intended to be used.”—(*Denman*.^a)

Levret and some other French writers have admitted its employment in some cases where the head was rather tight in the passage—to use their own words—“ on the point of being enclavée,” but not when impacted.

“ Il resulte de toute cette théorie-pratique, que la levier de R— ne peut être d'aucun utilité pour degager du passage un tête reellement *enclavée*, dans le cas pour lequel on propose cet instrument, cas auquel on le borne absolument, et que bien loin que cet instrument, tout simple et tout ingenieux qu'il est, puisse être utile pour ce cas, il ne peut manquer au contraire d'être nuisible à la mère et à l'enfant, surtout en suivant le manuel decrit, et même quoique l'on fasse usage de la cordelette, de la maniere que nous avons pensé que Roonhuisen s'en servit, tandis que les *forceps* y sont d'une tres grande utilité: mais afin que l'on ne puisse pas nous soupçonner de partialité, n'importe par quels motifs, nous allons exposer un cas pour lequel la levier de Roonhuisen peut sans danger être de quelqu'utilité.”^b “ I presume it appears from what has been said of Roonhuisen's lever, that from its figure and manner in which practi-

^a Introduction to Midwifery, p. 291. ^b Levret *Accouch. laborieux*, p. 291.

tioners made use of it, it must seldom be of any use in extracting the head in *difficult* labours, and that in general the frequent attempts to deliver in these cases with this instrument must be highly injurious.”—(*Dease*.^a)

I have deferred until now stating the two principal conditions of its employment, even in these cases, viz. the presence of labour pains, without which there would not be a chance of success : and the dilatation of the os uteri.

“ You must never attempt to use the vectis when the uterus does not act : for it is a powerless instrument, and only adapted to cases in which the pains are rather languid, than altogether deficient.”—(*Gooch*.^b)

3. The case which appears to me most suitable for the use of this instrument, and in which the probability of success is greatest, is that which I have sketched at the commencement of this essay, when the head having descended into the pelvic cavity is arrested in its progress, not by any mechanical impediment, but by the inefficiency (not absence) of labour pains, and when the patient is beginning to show symptoms of constitutional or local disturbance. This condition does not take place until the second stage of labour has lasted some time, and as after these symptoms show themselves, there is danger to the patient in further delay, it is important to obtain aid.

“ In this most favourable presentation,” says Dr. Breen,^c “ the uterine action is occasionally for hours exerted in vain, from causes which we are frequently unable to account for. Much delay may excite fears for the safety of the child, and lay the foundation of a tendency to inflammation in some of the soft structures of the mother, indicated by some one or several of the following symptoms : increased frequency, or fulness of the pulse ; tongue loaded in its centre ; secretion of urine diminished and becoming higher in colour, sometimes requiring to be drawn off

^a Obs. in Midwifery, p. 44.

^b Lectures by Skinner, p. 214.

^c Dublin Journal, vol. vii. p. 364.

by the catheter ; countenance assuming an anxious aspect ; stomach irritable ; general increase of restlessness."

Now as there is supposed to be space enough, and pains though feeble, a very little additional force will often succeed in bringing the infant into the world at once. As there is nothing in the nature of the instrument to add to the danger, and especially as the tractile force will probably be sufficient, it seems peculiarly suitable, and I may add, from all the testimony I can collect, that its success equals our expectations.

4. In cases of convulsions or other accidents occurring during labour, provided only that labour pains continue, the assistance of the lever may be sufficient to terminate the labour.

As to the *time* when the instrument may be most advantageously used, I may adopt the words of Mr. Dease : " It requires a certain degree of cool discernment, which I believe is only acquired by long practice, to know when a woman is still capable of assisting her labour, or when the head is sufficiently low in the pelvis to use the extractor."^a

If the object desired, be to aid the head in passing through the upper outlet, or to rectify its position there, it will be well to operate so soon as the os uteri is dilated or dilatable.

When the head is in the pelvis, it is desirable to have it as low down as may be, as the operation is then much easier.

" Under these circumstances, I think it best to examine the woman as she lies on her side : if the surgeon finds that the head is sunk deep in the pelvis towards the sacrum, at least one half, he may apply the extractor : he should not form his judgment of the descent of the head from examining towards the pubis, for here, from the shallowness of the pelvis, and the swelling of the scalp, he will be very apt to be deceived, and imagine the head to be much lower down than it really is."—
(*Dease*.^b)

In coming to a conclusion on this point, however, regard must

^a Observations in Midwifery, p. 47.

^b Observations in Midwifery, p. 49.

be had to the constitutional symptoms; if these be urgent, it would be unwise to lose time after the stage at which the vectis may be easily applied.

The occurrence of any of the accidental complications will in each case determine the period for operating, according to the urgency of the symptoms.

I regret much not having any *statistical results* to submit to the Society, but in this, as in too many other cases, practitioners seem to have concluded, that as the instrument is said to be quite safe, it was therefore useless to record the fact.

De Bruyn is said to have used it successfully 800 times in 42 years.

MM. Titsingh and Berkmann used it 262 times in 24 years, and saved 80 or 90 children in the 100.—(*Herbiniaux*.^a)

As to the *comparative results*: the *alternative* of the vectis is the forceps, and their respective merits have been the subject of controversy with most writers who have treated of them. Upon reading over the different sides of the question, it would seem that each writer has taken up the subject too much as a partisan. To compare their utility in certain cases is little more than a waste of words, as for example, where the pains have ceased, or where compression is required to make the head of the child pass. In such cases the vectis is of no use, and it would be highly reprehensible to use it. But where there is room, and when the pains persist, there the vectis being sufficiently powerful has this signal advantage, that there is but the one blade to be introduced, and but the thickness of one blade added to the child's head. It is possible that the single blade may be able to act where the bulk of two would render extraction impossible. These appear to me to be the peculiar advantages of the vectis, and therefore I shall not detail the controversy more fully, but refer you to the works of Osborn, Bland, Denman, Ramsbotham, (Lectures,) Burns, Conquest, Herbiniaux, Camper, Levret, &c. &c. One point, however, I must notice, which

^a Vol. i. pp. 65-6.

has been urged as an advantage in favour of the vectis, viz. the secrecy with which it may be used. Now this I consider a decided disadvantage. I most fully agree with the opinion of Dr. Osborn, and shall make no apology for transcribing it at length, as it applies forcibly to all midwifery operations.

“ In the first place I am persuaded, that if concealment in the use of the means intended for relief in laborious or difficult labours be not permitted, but that the absolute necessity of such means be first established, and that every practitioner be obliged openly and avowedly to use them, we should never again hear or read of one person having used the vectis in 800 and another in 1200 cases (Van Swieten, Camper, and Herbiniaux.) Nor shall we again hear of the great number of women which some practitioners are constantly boasting of having delivered; for no man can attend a great number of women in labour, in the manner he ought, in the way nature demands, or a conscientious discharge of his duty requires. Nor do real difficulties occur so often, as to render it possible to believe, that any man's life could afford such numbers of difficult cases, as are stated in the printed accounts from abroad. As I feel thoroughly convinced of the propriety and necessity of a fair and candid avowal of the use of instruments, in every case of midwifery where they are to be employed, so I must insist that their concealment cannot be justified by any proper motive. Such an open avowal implies a conviction in the practitioner's mind of that irresistible necessity for their use, that supersedes every other consideration: it implies a consciousness of the rectitude of his conduct, and it implies a voluntary acceptance of the consequences of the operation, which ought to make part of his professional duty: and it clearly demonstrates to the satisfaction of the patient and her friends, that no motive of convenience to himself could urge him to an operation, which may prove ruinous to his own reputation and interest. Besides, not to insist upon that responsibility from the operator, is to deprive the patient of the best and surest security against a precipitate

performance of the operation. If once the practitioner can rest assured, that let the event of the case be ever so unsuccessful, the injurious effects of his operation will be buried in eternal oblivion, by blending the mischief arising from the indiscreet use of instruments with the natural consequences of labour, he will certainly have nothing to weigh against the tempting advantages of convenience or emolument to himself; but while he is shortening the duration of the most irksome part of his professional duty, the waiting upon a slow and lingering labour, he will flatter himself that by delivering, he is doing an acceptable service to his patient, in shortening the duration of her sufferings.^a

Mode of operating.—Premising then that the case is one adapted for the vectis, that there is space enough, that the os uteri is fully dilatable if not dilated, that there are pains,^b and that the patient and her friends have been made acquainted with our intention, it next remains for us to consider the method of using the instrument;^c

1st. As a lever, and

2ndly. As a tractor.

1. *As a Lever.*—The first point to be decided is, over what part the instrument is to be applied; and here we have latitude enough. “Some,” says Dr. Gooch, “apply it over the occiput; others behind the ear, by which it has a bearing against the prominence of the mastoid process; and others against the chin. The two first are perhaps the best when the head is high, as

^a Osborn's Essay on Midwifery, p. 144.

^b Dr Burns suggests that if the pains be weak, it may be advisable to give the ergot previous to applying the instrument. Principles of Midwifery, p. 495, note.

^c I need hardly repeat what has been so forcibly recommended, that in all midwifery operations it is desirable to have the benefit of the advice and assistance of another practitioner. I recollect that the late distinguished Professor of Midwifery at Edinburgh, when speaking of operations, always reminded his pupils that they *might* have to defend their conduct in a court of justice, and that therefore it should always be such as would justify them before a jury of midwifery practitioners.

considerable force is required to move it, which may be employed with more safety against either the occiput or mastoid process than against the chin. But when the head is low down, resting on the perineum, less force will be necessary, and the vectis may then be applied against the chin; but the instrument requires to be used with great caution lest the jaw should be injured.”^a

“When the crown of the head presents, the fixture of the blade is generally near the situation of the mastoid process, or towards the occiput. The last has the advantage of sooner rendering the position of the head properly oblique. In face cases the lever passes in a line from the forehead or root of the nose, its extremity resting on the occiput between the vertex and neck, but scarcely so far back as the vertex.”—(*Burns*.^b)

“In employing the vectis then, we shall find it necessary to apply it over different parts of the cranium, and perhaps the face also successively, in order to relieve the head from its fixed situation, and favour its descent; and we may sometimes feel it necessary to use it one minute as a tractor, and the next as a lever; being, however, most cautious to *make the fulcrum of our own left hand* as first recommended by Pean.”—(*Ramsbotham*.^c)

De Bruyn applied it over the mastoid process; Camper over the lower jaw; Lowder on the forehead, &c.—(*Mulder*.)

I have already pointed out the temptation to make the soft parts of the mother the fulcrum, and the mischiefs which result. As far as my judgment extends, it would seem that the vectis ought never to be used as a lever of the first class; even as one of the second class much caution will be necessary.

“When an instrument of this sort is used, it is proper to make the hand the fulcrum on which it acts: now if the force required is but small, this may certainly do well enough, but

^a Lectures by Skinner, p. 214.

^b Principles of Midwifery, p. 495.

^c Lectures, Medical Gazette, May 31, 1834, p. 309.

where great force is required this is a very bad support ; besides the bony parts of the pelvis lie so convenient, that we may rest our instrument on any part of it. Yet we should recollect, that whatever part we convert into a fulcrum we injure more or less according to circumstances. If we apply it over the symphysis pubis we press upon the urethra ; or if in other situations, we shall injure the clitoris or vagina.”—(*Dr. John Clarke.*^a)

“ The injuries inflicted indeed must have been frequent and great, and this led Pean, in 1772, to suggest the possibility of delivering by the vectis without making a fulcrum of the mother’s structures. He proposed a practice, which is now sometimes adopted, of grasping the shank of the instrument with the left hand—the outer edge of the little finger being applied towards the vulva—making that hand the fulcrum, and pressing the extremity of the blade on the child’s head, by raising the handle firmly on the right.”—(*Ramsbotham.*^b)

“ Introduisez toute la cavité de l’instrument, soit le long du front de la tempe ou de l’occiput, dans la matrice avec la main droit, presque à ce que vous sentiez que la cavité répond à la convexité de la tête, alors il passera l’oreille et se posera à côté du cou, et le bout vers le menton de l’enfant plus ou moins, selon la grandeur de la tête : levez alors l’autre bout, puis appliquez la main gauche vers le milieu de la spatule, déprimant ainsi, et tirant la tête en même temps en enbas elle sera délivrée dans un instant.”^c

Having determined on what part of the infant the lever is to be applied, the instrument is to be well warmed, and greased or soaped, and the patient placed in the usual position for delivery, on her left side ; the operator is to introduce one or two fingers of his left hand to serve as a director for the vectis, which is to be carefully and gently passed over the convexity of

^a London Practice of Midwifery, p. 208.

^b Lectures in Medical Gazette, May 31, 1834, p. 307.

^c Mem. de l’Académie de Chir. Baudelocque, vol. ii. p. 47.

the child's head, until it has reached the point to which the force is to be applied.

"This attained, the handle should now be held firmly with the right hand, while the index and middle finger of the left, fixed about two inches from the screw part, within the vagina, become a fulcrum. On this fulcrum or point of support, the instrument is made to move from the sacro-iliac symphysis towards the hollow of the ilium, by the action of the right hand on the handle. In this way it describes the section of a circle, and glides on to the occiput. Should the occiput point to the right ilium, the left hand must be employed; if to the left ilium, the right hand must be used. When a pain takes place, the accoucheur should gently aid it by drawing down in the axis of the pelvis (of the upper outlet). In this way the occiput is depressed, while the chin approaches the child's breast, and the head is reduced to the smallest compass, and is thus enabled to pass through the cavity of the pelvis. As soon as the occiput is brought so low as to press on the perinæum, the instrument should be withdrawn, and re-introduced with the usual precautions. The object now in view is to place the instrument over the face of the child. To effect this, the hand must be passed up, as at first directed, to the right or left sacro-iliac symphysis, according to the situation of the face. When the instrument gets above the brim of the pelvis, a finger or two must be inserted by the side of the instrument, and pressed on till it passes over the forehead into the face, so as to embrace the chin. The practitioner has now nothing to do but to draw down during the time of pain, increasing the power according to the degree of resistance."—(*Gaitskell*.^a)

Or if we prefer it, the right hand, grasping the handle, may be made the fulcrum, and the force applied by the left hand at the junction of the blade and handle, directing it downwards and backwards until the descent of the head is accomplished.

^a London Medical Repository, November, 1823, p. 380.

“ If the instrument should slip, a fresh purchase must be obtained. As the head passes over the perineum the efforts may be relaxed ; and if the pains appear sufficient, it may be withdrawn altogether, and the termination left to nature.”—(*Breen*.)

2. *As a Tractor*.—The preliminary steps, introduction, &c., are the same as when it is used as a lever ; but instead of making use of one hand as a fulcrum, both hands are employed in the one office of maintaining a firm purchase, and drawing downwards and a little backwards during the pains. The effort is to be relaxed during an interval ; and this alternation of traction and rest is to be continued until the head has descended to the inferior outlet. As before, it may be allowed to pass over the perineum without assistance, if the pains be adequate to its expulsion.^a

There is, I believe, no *danger* to the mother or child when the vectis is in skilful hands, but in those of the ignorant or inexperienced great mischief may be done.

1. It may be introduced before the os uteri is dilatable : “ of this error contusion, laceration, and death, may be the consequences.”—(*Blundell*.^b)

2. “ By an incautious mode of passing the instrument, the parietes of the uterus may be ruptured.”—(*Gaitskell*.^c)

3. “ By employing the extracting power, without bearing in mind the different axis of the pelvis, and the position of the fœtal head in relation to those axes,” (*Gaitskell*,^d) the lever will be insufficient, and the mother injured.

4. By passing the instrument outside of the uterus instead of within its cavity, a fatal wound may be caused.

5. “ By exerting the power without regard to the pains, the operation will be in vain.”—(*Blundell*.)

6. By making a fulcrum of the soft parts of the mother, much injury may result.

^a See Dr. Breen's directions, *Dublin Journal*, vol. vii. pp. 365-6-7.

^b *Principles and Practice of Obstetricy*, p. 516.

^c *London Medical Repository*, November, 1823. ^d *Outlines*, p. 103.

"The lever or vectis is a very powerful, and consequently a very dangerous instrument, if it be used on lever principles, acting upon and injuring the soft parts of the mother, at the fulcrum or point of support. In the hands of men who have not employed it rather as a hook than lever, it has done incalculable mischief."—(*Conquest*.)

"Mais souvent l'urethre en est fort endommagée, souvent le perinée defend plusque dans l'accouchement naturel, et que lors'quon se sort d'un forceps quelconque."—(*Camper*.^a)

7. By exerting too much force as the head passes over the perineum; or neglecting to support it, "you may tear the perineum, so as to lay the genital fissure open into the anus."—(*Blundell*.)

8. By making too much pressure with the point of the instrument upon the part of the child to which it is applied, a wound may be inflicted.

"We will now consider what circumstances will arise from use of the instrument, and first the point of action, which is the head of the child. It is too obvious to need mentioning, that the force applied by the instrument must be equal to the resistance, if not superior to it; and then the mischief may arise to the parts of the child's head so acted on, producing much injury. The ear may be injured: the lower jaw or zygomatic process of the temporal bone may be broken; or any part of the surface from the pressure may slough off. These evils are by no means imaginary; there are various instances recorded of each of them, and that under the hands of the most careful and dexterous men."—(*Dr. John Clarke*.^b)

The subsequent *treatment* varies very little from that required after ordinary labour; there is very little shock, and no injury if the operation be skilfully performed. The parts should, however, be carefully examined, and if necessary, a spirit lotion applied. The same treatment should be applied to the head of the child, if the instrument have bruised the integuments.

^a Mem. de l'Acad. de Chirurg. vol. xv. p. 225.

^b London Practice of Midwifery, p. 208.

ART. XVIII.—*Letter from* DR. CHURCHILL.

TO THE EDITORS OF THE DUBLIN JOURNAL OF MEDICAL SCIENCE.

GENTLEMEN,

My attention having, a short time ago, been directed to a little work, called "*The Accoucheur*," by a surgeon of Paisley, I found that by the quotation of a sentence from one of the medical periodicals as mine, the author had attributed to me an attack upon Dr. Hamilton of Edinburgh, for whom I have the greatest respect ; and with whom I was at the time in friendly correspondence. As such a supposition was extremely painful to me, I wrote to the author, pointing out the misquotation and the erroneous inference, and requested that he would oblige me by correcting it. He at once, in the most prompt and gentlemanly manner, wrote me the following letter with permission to publish it at my discretion. I therefore beg that you will have the kindness to give it a place in your next Number.

I have the honour to be, Gentlemen,

Your most obedient Servant,

F. CHURCHILL.

136, Stephen's-Green, West,

Nov. 6th, 1839.

TO DR. CHURCHILL.

SIR,

In my work "*The Accoucheur*," p. 252, I have committed an error in attributing to you, the sentence beginning "and as for poor Professor Hamilton," &c. which is appended to an extract from your Report of the Western Lying-in Hospital, in the *Med. Chir. Review*, for Oct. 1838.

The best apology which I can make for this and other unintentional errors, is to refer you to page 1 of the preface, and p. 216 of the work, under the head of Concluding Observations. I exceedingly regret that any expressions used by me, should have produced an impression of a personal nature, either regard-

ing yourself or any other. I solemnly state that the sole object I had in view when writing the book, was the improvement of the practice of midwifery.

I ave the honour to be, S ir,

Your most obedient Servant,

JOHN CRAIG.

P. S. After the correction of this error, the phrase “an attack upon Dr. Hamilton,” used by me, is in my view no longer applicable to you.

ART. XIX.— *Observations on the Effects and Mode of Application of Remedies.* By JONATHAN OSBORNE, M.D., Physician to Sir Patrick Dun’s and Mercer’s Hospitals, &c.

THERE is no department of medical science, which is in so imperfect a state as Pharmacodynamics, or the knowledge of the effects of medicines. The cultivators of the materia medica, of late years, have been led to consider medicines with regard to their preparation rather than their medical effects. No doubt great improvements have been effected in the chemical department, the proportions have been accurately adjusted, and in the natural history of the materia medica, much error has been removed, while new preparations of the greatest value have been added, and the Colleges are periodically engaged in an honourable rivalry in contesting for the palm of excellence in their Pharmacopœias. When however we ask what progress has been made in obtaining a knowledge of their effects and mode of action, the answer must be, that instead of accurate observations, we have vague statements, at first in praise, and afterwards in condemnation of remedies, so that between the assertions of advocates and accusers, the seeker of truth is at a loss how to decide; the compiler of books decides by taking the mean proportional between the conflicting statements, but the science itself, after years so spent and multitudes of volumes so written, is found to have gained nothing in respect of positive knowledge.

It is then to be hoped that individuals who have opportunities of observing the effects of medicines, and of comparing the merits of their preparations, and who at the same time are free from those influences which produce partiality, will contribute to the stock of actually ascertained facts. As evidence on these points is only valuable in proportion as it is *cumulative*, this branch of knowledge must ever require a number of co-operators. Hence I am induced to commence a series of communications in the valuable pages of this Journal, which shall have for their object to record detached observations on the effects of remedies, and on their preparations and peculiar qualities, in the hope not only of bringing forward propositions which will be both true, as well as new, but also in the hope of inciting others to record their evidence on the same subjects.

HEMLOCK.

Even the extract, imperfect as it is, has an effect in appeasing the pain in cancerous affections of the uterus, and that without exerting sensible narcotic powers, which almost excuses Stoerk for the error into which he fell in proclaiming it as a cure for cancer. I have applied it externally, and given it in such cases sometimes without effect, but sometimes with remarkable alleviation of pain after opium had failed; and never observed any ill effects except in one case of a woman labouring under scirrhus uteri, who obtained great relief from pain by it, but when the dose was increased to four grains three times daily, had headach, black motes in vision on sitting up, and saw two persons instead of one; all which disappeared when the remedy was discontinued.

The uncertainty of the extract may be shewn by an easy experiment. The conine, which in the process of decomposition is partly resolved into ammonia, is in this preparation always, according to Professor Geiger, more or less deficient, and often entirely absent; and to prove that this decomposition has taken place, it is only necessary to add some water of caustic potash,

when the ammonia may be distinguished by the odour, and by holding over it a rod dipped in muriatic acid, whereupon the fumes formed by the muriate of ammonia are rendered visible. This experiment has shewn ammonia in every specimen of the extract in which I tried it.

Wishing to secure the fresh plant in a state of preservation for winter use, I resorted to a plan which I believe will be found applicable to the preservation of plants generally for medical purposes. I caused the leaves and smaller stalks, fresh gathered before flowering, to be pounded up and intimately mixed with an equal weight of treacle. This mixture, of the consistence usual in an electuary, continued for several months until used, without shewing the slightest tendency to decompose or to change any of its sensible qualities. This can only be ascribed to the treacle,* and I suggest this as a very useful mode of preserving as well as of exhibiting those vegetable productions, the efficacy of which depends on their freshness. I gave this preparation in six grain doses to several patients, in whom I thought it might prove servicable, but its effect was quite different from what I had anticipated. In every instance it acted as a purgative, producing full dejections, without either nausea, tormina, or narcotic symptoms. There is nothing in this fact inconsistent with the opinion of those, who maintain that the poison which Socrates took was the juice of hemlock. Unless the narcotism from a large dose was so overpowering as to stop the ordinary susceptibilities of the intestines a very different result might be expected from that described to have taken place before his death; but it appears from Theophrastus, that the Athenians usually mixed the juice of poppy with the hemlock intended to poison criminals; which suffices to

* I had an opportunity of seeing an open vessel of treacle which was known to have lain exposed to the air in a store room for years, but which was the same in quantity and quality as when plac'd there. It neither formed crystals, admitted the growth of cryptogamous vegetables, nor evaporated. Early this summer, I placed a vessel of treacle exposed to the open air; it now, November 26th, remains exactly as I left it.

explain not only the easy mode of death, but also the absence of purging in that particular instance.

Another consideration with regard to this important plant, is the great difference of its effects when gathered in different climates. Morris found the extract prepared in Portugal much more efficacious than that of Vienna; and according to M. Larronture, the best hemlock in France is that collected in the southern provinces. It has even been observed that when growing in southern aspects it has more activity than elsewhere.

We cannot expect that in regard to the place of growth, uniformity will ever be attained, but in regard to the part of the plant to be used, it is manifestly of great importance that a proper selection should be made. I think that, for external use, the entire plant, preserved in the way I have mentioned, would answer well, being both cheap and convenient; but that for internal use the seeds should be preferred; 1st, on account of their uniformity. Let it be remembered that seeds are, of all parts of plants, the least liable to variations in their mode of growth, structure, and chemical composition, and that they are the least dependant on artificial modes of drying for their future preservation, seeing that that process is performed by Nature herself on fixed and uniform principles; and on this head I cannot do better than refer to the seeds of *Colchicum*, which have enabled us to act with that plant, as previously, when the root alone was employed, might have been attended with danger: 2ndly, because the seeds of hemlock contain more of its active principle than any other part. For this I refer to the experiments of Professor Geiger, who found that six pounds of the fresh seeds contained about an ounce of conine, while a hundred pounds of the plant only afforded a drachm. I regret that the unfavourable state of the last season prevented a collection of the seeds, in sufficient quantity to enable me to experiment adequately on them, but I think enough has been said to shew that the tincture of the seeds would be as great an improvement for hemlock, as it has been for *colchicum*.

SCAMMONY.

I find in my notes, that several years ago I experimented on the scammony in use at Sir Patrick Dun's Hospital, which establishment is always supplied from the most respectable sources. Half an ounce of scammony was treated with ten parts of rectified spirit, and formed a light straw coloured tincture, which was insipid to the taste, but left after it an acrid sensation in the pharynx.

Of the scammony employed, two drachms and eighteen grains were dissolved. This tincture was evaporated and a residue obtained, which was transparent, of a dark yellowish colour, and of resinous lustre and fracture. It was given as follows, with the following effect.

Rose Delany (phthisis) has been usually purged by castor oil; took four pills of four grains each, at intervals of three hours. Had two dejections, one at night, the other this morning. Slight tormina after the first pill.

Brig. Corcoran (ulcer of leg) took four of the same at same intervals. One dejection this morning. No tormina or nausea.

Mary Bias (hepatitis), four pills as above. Began to operate about six hours after first taking. Nine dejections during the night. No griping, but some slight nausea.

Brig. Corcoran (bis) four pills as before. Operation commenced in seven hours after first taking. Four dejections without tormina or nausea.

The alcoholic extract was treated with cold water, and yielded a small proportion of extractive, which was found to possess no purgative quality; but when heated, emitted the peculiar odour resembling that of old cheese. It had a sweetish but slightly acrid taste, and was deliquescent.

The part insoluble in alcohol was treated with water. After digestion for several days, it yielded about six grains of gummy insipid extract, which had no effect as a purgative.

The residue insoluble both in water and alcohol weighed

fifty-six grains, had no purgative effect, and appeared principally to consist of gravel.

The above examination being rather medical than chemical, it is not necessary to apologize for the want of minuteness in the measurements. It is sufficient to show that the purgative quality of scammony resides exclusively in the resin. Hence the tincture would be a useful and efficient medicine, being nearly tasteless, and by adding it to syrup of roses or of ginger, it could be given to children without its presence being perceived.

THREAD SETONS.

The ordinary seton, although a measure of great value when it is desired to keep up a permanent counter-irritation, is yet often attended with much unnecessary pain and inconvenience. Especially in the nape of the neck, the size of the strap of threads or of gum elastic introduced into the opening is productive of general discomfort and teasing, not well calculated to diminish a tendency of blood to the head, but rather the contrary. I have adopted a plan which will be found better suited to many of such cases. It is to make a seton with an ordinary sewing needle of the thickest kind, and one thread of oiled silk. This is passed through a piece of the skin held between the finger and thumb, about six inches of the thread being allowed to remain. In twenty-four hours considerable redness comes on, and in a few days a purulent discharge is set up, much more in quantity than a comparison between the size of it and of the ordinary seton might lead to expect. The opening gradually enlarges, and no doubt in process of time, like the perforations made in the ears for ear-rings, assumes the function and secretion of a mucous membrane. The trifling degree of pain, however, inflicted by the operation enables us to multiply those setons, and to substitute new for old ones, so that I think it is evident, that in this way a greater discharge and a more efficient counter-irritation may be maintained, with less inconvenience than by the ordinary setons, and in places where the former would be impracticable.

BELLADONNA.

There is one property of belladonna, which I mentioned in a medical report of Sir Patrick Dun's Hospital in 1831, and which it has proved itself to possess in every instance, without exception, since that time ; so that it is unnecessary to detail cases on the subject. It is this, that *it causes an immediate cessation of the migratory or flying pain of rheumatism, without producing any effect on the fixed pains.* In this way it acts like a charm both in acute and chronic cases, and when it is recollected that in acute rheumatism, especially, the greater part of the suffering, and that most refractory to ordinary treatment, is the perpetual shifting of pain along the direction of the muscles from one joint to another, its value in all such cases will at once be admitted. The dose I give is one-third of a grain, thrice daily, increased to a half a grain every third hour. After trying various combinations, I prefer to give it in simple extract of gentian, as much as is sufficient to secure its accurate division into pills. Having observed its efficacy to be confined to the pains shooting along the direction of the muscles, and that any abatement of pain which it produces in neuralgia, or where the pain pursues the course of the nerves, is of a very inferior degree, and attended with great uncertainty, it appears to me that it acts on the muscular fibre belonging to the voluntary muscles as on the iris, and by stopping the spasm which is always present in severe cases, causes a cessation of the peculiar pain. This kind of pain always resembles fatigue, causing general restlessness and inability of remaining long in one position ; and suggests very much the sensations experienced after excessive muscular fatigue, when the spasms, not of entire muscles, but of fibres, prevent sleep, while at the same time they produce a feeling of intolerable weariness.

EMETICS OF IPECACUAN IN HÆMORRHAGE.

Having already (Trans. of the Association, vol. v.) stated the uniform success attending this treatment, I should not repeat it

here, were it not that some authors, who have subsequently treated professedly on diseases of females, have entirely omitted to mention it. I have only to add, that with me it has never as yet failed, except when the progress of the case afterwards proved the formation of scirrhus or cancerous structure. The remarkable effect of emetics of ipecacuan in restraining hæmorrhage, is not confined to this organ. In a case of violent epistaxis, in which several remedies were ineffectual, I tried it while preparations were going on for plugging the posterior nares, and with success, so as to render that measure unnecessary. In hæmoptysis, I am unable to add to the facts already known respecting its efficacy, being of opinion that hæmorrhage from the lungs is always salutary, and that the practice of giving the mineral acids, &c. to discourage it in phthisis is injurious. A very considerable benefit is generally perceptible, after the vessels of the diseased lung have been unloaded by this discharge. When, however, a violent hæmorrhage takes place from the lungs, and blood is expectorated in such quantities as to endanger life, then all our efforts must be directed to its suppression. In a late case (not phthisis) I failed with the emetic, but as I lost sight of the patient subsequently, I am unable to pronounce as to the cause of the hæmorrhage, and therefore as to the cause of the failure.

SUPPOSITORIES.

When substances are introduced into the rectum, by the simple instrument, consisting of a tube and moveable rod inside, very different effects may be expected from the same, when introduced *more majorum* by the finger. In the former case it is lodged above the sphincter, and much less mechanical disturbance accompanies it than in the latter, which is in all respects a detestable process. In irritations of the bladder, or painful affections of the uterus, two grains of watery extract of opium with six of soap—or, in spasmodic states of the urethra, a grain of extract of belladonna, are attended with marked benefit, and are productive of no inconvenience. It has

been an old established practice among nurses, in case of refractory children, to introduce a fragment of soap with the finger, and generally, the necessity of giving purgatives is in this way for the time avoided ; but on consulting authors, I find no account of purgative suppositories for adults, except some intended to cause the expulsion of ascarides from the rectum,* since the time of Hippocrates, who is said to have composed them of salt and colocynth.

Being desirous to ascertain, to what extent purgatives would act, when introduced into the rectum in the form of suppository, I in the first instance fixed on assafoetida, and ordered it to be introduced by the tube and rod in eleven cases. It succeeded in all, producing one or two dejections, except in two in whom it failed, and in one individual, who suffered much from flatulent distention of the colon, it was attended with relief much superior to that afforded by other purgatives. This may be ascribed to the volatility of the agent, which, diffusing itself through the colon, produced a general stimulation and contraction of the same, and being in the solid form, did not expend its volatile parts with so great a rapidity as to cause its premature expulsion, as occurs in the case of the foetid enema. Hence in the numerous hysterical and dyspeptic affections, in which great annoyance is experienced from the pressure of an elastic tumour in the left hypochondrium, consisting of the arch of the colon distended by gas, beyond its capability of contraction, much benefit may be derived from the prolonged presence of the vapour of assafoetida, and from a perusal of the experiments related by Joerg† to have been performed with this substance, it appears the best adapted for producing an equal and universal contraction of the gut, the want of which is evidently a main cause of the distressing sensation just mentioned.

* Ganbii de Methodo concinnandi Formulas, Lug. Bat. 1767, p. 433.

† Joerg Materialien zu einer kunftigen Heilmittellehre, p. 366. This excellent work is well named "Materials." I hope to make frequent use of the experimental portion as such, but while placing an implicit reliance on the facts, I shall have to offer different explanations of them, from those given by the able author.

Suppositories consisting of one grain of elaterium in nine of soap, produced in two cases four dejections, in two caused one full dejection, and in one failed.

A suppository of two drops of croton oil added to soap, in a case of obstinate bowels, caused three full dejections, commencing in about ten minutes after its insertion. On a repetition, it caused the expulsion of one hard fæcal lump, and was followed by tenesmus. In another case, I formed a soap of croton oil and caustic potash, and having dissolved it in a large quantity of water, ordered it to be injected as an enema. Those observations, in connexion with some made previously, tend to shew that croton oil is not well suited for this purpose, and that if retained, it may cause serious irritation of the rectum.

THE WIDOW WALSHE'S PILLS.

Those pills have an extensive sale in England, and are in high and long established repute as emmenagogues. One of them was given to a young lady under my care, with the desired effect following on the same night, and three other cases came to my knowledge in which a similar result followed their use. I procured some, and subjected them to the following observations; first, they are nearly tasteless, and have no odour; second, they have no purgative effect; third, on exposure to the blow-pipe a slight charring took place, and they were resolved into a red powder, (with very slight loss of weight;) this when treated with repetitions of dilute muriatic acid, was dissolved, (without effervescence,) and the solution struck the blue colour with ferrocyanide of iron; fourth, on treating another pill with water, its colour was scarcely altered, but it threw down an abundant white precipitate with muriate of barytes. From the above I think it follows, that they are composed of sulphate of the peroxyde of iron, with a small quantity of insipid vegetable matter, probably gum, as much as requisite for adhesion. It became a ques-

tion to ascertain the effect of the same salt of iron in the same dose as in those pills, but the results of several observations have not been consistent or satisfactory. On the causes, however, which render the result of emmenagogues uncertain, and on the best mode of eliciting the discharge under various circumstances, I have some facts to state, which shall appear on a future occasion.

ON MEASUREMENT OF MEDICINE IN PRIVATE HOUSES.

It often happens that a fluid medicine, which the practitioner intends to be taken in the course of two days, is finished before the conclusion of the first, or *vice versa*. The different sizes of spoons, glasses, and indeed of all household utensils, render them most uncertain and unsuitable measures for medicines; and yet they are those most commonly employed. I have carried the following plan into effect, and find it easy of application and perfectly intelligible to patients. The entire mixture is directed to be divided into a certain number of equal parts by marks on the side of the bottle, which being usually of the same diameter from top to bottom, is readily done by a strip of paper affixed at one side, on which the divisions may be marked with a pen. Thus when a sixth or a fourth part of a given quantity is ordered, there is as great a certainty of the proper dose being taken as there is in the quantity of the ingredients of which it is composed, which are now all measured by means of glass vessels with marks on their sides.

A TASTELESS FORM OF IPECACUAN.

When it is desirable to administer ipecacuan to refractory children, or to persons to whom the vinum ipecac. is particularly odious, as is often the case, the following form will be found to answer.

℞. Rad. Ipec. contus. ʒss.
 Aq. bullient. q. s. ut f. colat. ʒ iss.
 Adde Syrupi Limon. ʒss.
 Sum. partem 12^m. 3tiis horis.

(*To be continued.*)

ART. XIX.—*Observations upon the Use of Ipecacuan in Internal Hæmorrhagies, illustrated by a Report of Cases successfully treated with it.* By W. ADAMS TRENOR, ESQ.

IF there is one form of disease more than another which demand prompt and decided measures for affording relief, it may with safety be said to be hæmorrhage. It is not the object of this paper to enter into the causes or consequences of hæmorrhage when it occurs in any of the internal organs; medical readers will find abundant information in the numerous authors who have treated this subject very largely, thus showing the importance which they attributed to this form of disease. The article of *Materia Medica* which is mentioned at the head of this paper, has long held a place as a remarkable agent in restraining internal hæmorrhage. I may mention the five cases of menorrhagia, reported by Dr. Osborne of this city, which were successfully treated by emetic doses of it; (*Transactions of the College of Physicians*, 1828;) also in the fourth volume of the same work, some remarks upon *Hæmatemesis*, by Dr. Sheridan, in which both he and his father had used this medicine with the same happy results which attended Doctor Osborne's cases of menorrhagia. The year following the publication of Dr. Osborne's cases, I was called very early in the morning to see a young gentleman about 20 years of age, very robust, being six feet four inches in stature, and well made in proportion; had always enjoyed excellent health. At the time of my visit, about 4 o'clock in the morning, I found this young man sitting with his feet upon the hearth-stone, and his hands immersed in a basin of cold water. The floor round where he was sitting was covered with florid, frothy blood, and a large vessel in the room almost filled with it. He felt faint and suffocating; at times finding his inspirations very difficult, from the great quantity of blood which was literally flowing from his mouth. His face was a little flushed, which was naturally ruddy; his voice weak, and indistinct; pulse quick, small, and rapid; could not

bear the slightest attempt at the reclining posture; called for air, and had all the windows and the door open. This was no case for venesection, and to wait for the sedative influence of the acetate of lead and opium, did not appear safe; it was instantly determined in my mind to use the ipecacuan, of which I gave ten grains in a little water. It was incredible the relief which followed the nausea, and some slight efforts to vomit, which this dose of the medicine excited. The hæmoptoe now abated; respiration became more easy; and in some little time the reclining posture could be indulged in. This young gentleman after some months recovered perfectly, went to America, where I understand he suffered severely from Asiatic cholera, but ultimately recovered from it. Dr. Crampton saw this young man with me at his father's residence in Simmonscourt.

The second case of this kind occurred in a young gentleman of tall and slender figure, seldom sick, yet of delicate constitution. He was in his 21st year; had been confined to the house for a few days, with what was considered catarrhal fever, got from travelling for some miles on the outside of the coach. Like the former case it was very early in the morning, 3 o'clock, when the hæmoptoe came on.

I found him supported erect in the bed, with both windows open; he could scarcely speak. The quantity of blood, though very great, was not anything like the quantity in the former case, yet the prostration was much more alarming: countenance was contracted and death-like; the anxiety of manner very great. The strength had failed, and the interval between each expectoration of blood had become longer, yet the breathing was becoming more oppressed, and the distress more urgent. In this case the ipecacuan was given in doses of two grains every fifteen minutes in a little cold water. He had not long taken the second dose until some nausea was felt; and with it the respiration became less oppressed. The third dose of the medicine increased the nausea to some effort to vomit, but the stomach did not empty itself. The appearance of blood now decreased,

and soon disappeared, nor did it return for several weeks, and was then very moderate in quantity. This case ended very differently from the former, as it rapidly terminated with phthisis. Dr. Marsh attended this young gentleman through the whole of his illness with me from the day after the hæmorrhage.

The cause of the hæmorrhage in these two cases was very different, yet the ipecacuan was a most salutary and efficient means in each in restraining the flow of blood.

The third case which I am going to mention, in its course and termination closely resembled the first.

Mr. H —, about 22 years of age, had been out rowing, and on coming home ran quickly up stairs to dress for dinner; on getting into his room a fit of coughing came on, and with it large mouthfuls of blood poured forth. When I saw him, 5 o'clock, P. M., much blood was coughed up, and considerable prostration of strength, although obliged to observe the erect posture. The face was more flushed than natural, and the pulse was very quick and small. In this case I gave small doses of the ipecacuan, which created little more than nausea: the hæmoptoe soon disappeared. The right lung presented some of the phenomena of pulmonary apoplexy in the mammary region. Dr. Graves concurred in the propriety of having a seton formed over this part of the lung. The recovery here was steady and uniform, as this young man has been ever since in his usual robust health and strength; the seton was removed at the end of some months.

These three were the most severe cases of hæmoptoe I ever witnessed; many of a milder nature have been treated by the same medicine in variable doses, and always with the most fortunate result in restraining the hæmorrhage, one of which only I shall detail.

Last February I was sent for to see Mr. T —, aged 36; had been exposed to cold and wet; a cough followed this exposure, and continued for ten days or so, giving him no uneasi-

ness, as he said the cough never hurt him; he thought it would go away like many others on former occasions. He hated medicines, as they always made him sicker than before he took them. His pulse was quick, but soft and easily compressed; the skin moist and perspirable; the sputa almost all florid blood, and had coughed up about a teacup-ful. The countenance, which I understand is naturally pallid, is now a little flushed; he was sitting in a very warm room with a large fire. Upon examination the right lung gave evident signs of being gorged with blood, as the rattle was no doubt occasioned by it, while in other portions of the lung the respiratory murmur was comparatively clearer than natural. Some leeches were directed over this portion of the chest; and on their being removed, a blister to be kept on for eight or ten hours. The infusion of roses with additional acid, sulphate of magnesia, and tincture of digitalis, to be taken in doses of two tablepoons-ful every three or four hours; perfect quiet and silence, with a cooler apartment, were enjoined. In about a fortnight this plan of treatment seemed to answer very well, and he was again able to resume his business for some hours in the day, as it required little or no exertion. His cough was almost well at this time, as he said he would not notice it. Not long after this, excitement of temper with violent language soon reproduced the hæmoptoe, and that too in no very moderate degree. I was again sent for; the violence of the hæmoptoe by this time had subsided, and the cough occasioned by the presence of the blood in the air tubes was now pretty frequent; the continuing of which he seemed to dread very much, as he knew the bleeding would be increased by it.

I now prescribed the ipecacuanha for him in grain doses every half-hour, until he had taken three or four grains, when if any nausea was felt he was to lengthen the intervals; this he was reconciled to do; as he said he would prefer being sick rather than have the bleeding continued. In this way he took twenty grains of hippo in the course of thirty hours. No vo-

miting came on, but much nausea; the blood disappeared from the sputa. I told him now to leave town for some weeks, and take his pills with him: this he did; and although he attended closely to business, and was often exposed to vicissitudes of weather, he has had no return of the hæmorrhage.

The following case of menorrhagia, which occurred to me some years ago, shews the value of this medicine in this form of hæmorrhage, and as it bears evidence in favour of the practice so long ago recommended by Dr. Osborne, I hope it will not be deemed uninteresting. Miss L., about 35 years of age, at the time of this history living in Rathmines, has often had menorrhagiæ, which had its first origin in a fright, accompanied by great exertion, occasioned while driving with some children who were in her charge. The coachman had left the driving seat of the jaunting car, the horse taking flight was making towards a precipice where instant destruction must have awaited them; making one great effort Miss L. contrived to lay hold of the reins, succeeded in altering the course of the frightened animal, and finally to control his speed. This form of hæmorrhage was the immediate consequence of this fright and exertion, and for which the late Dr. Cheyne, together with Dr. Morgan, now residing in Bath, attended, and I understand from herself they considered her in much danger; she slowly recovered, subject, at the period of menstruation, to excessive losses, often requiring much care and attention; notwithstanding all this, Miss L. was a stout, well made woman, and rather embonpoint.

The circumstance which placed her under my care was also of an appalling nature. An elderly lady, a friend of hers, had caught fire, and in endeavouring to save the old lady and the house from being burned, she herself received much injury from the flames, as her arms were deeply burned; when assistance had arrived and all the flames were extinguished, the apparently lifeless body of her friend, with all the horrible effects of the fire on her face and head, produced the same effect as the

fright she was first subjected to. The quantity of blood lost was very great indeed, for it was in a pool under the bed ; the bed-clothes literally saturated with blood. The lower extremities, were cooled by the vinegar and water, which she had been in the habit of using in milder attacks of this hæmorrhage, were now quickly dried, and a warm binder firmly bound round the hips, a sponge pressed out of hot water and vinegar introduced into the vagina, a compress of linen, and the bandage to secure all. While I was thus employed one of the family were endeavouring to make her swallow some brandy with twenty grains of hippo in it ; almost every appearance of life had fled ; there was no pulse to be felt, and the extremities, arms, and legs cold as death ; she scarcely breathed for an hour, during which time every exertion was made in the way of warm friction, turpentine enemata, and brandy, in such quantities as could be got down the throat. The hæmorrhage from the vagina seemed to have ceased, so far as the clean bandages gave information ; indeed death seemed to have closed the scene. At last the chest heaved, respiration now became evident, she could swallow better, and the brandy was more liberally given ; after much watching and attention, warmth seemed to become more evident on the surface. In three hours from her getting the ipecacuan, some slight effort to vomit came on, which we thought was occasioned by it. An eminent surgeon, who is now no more, saw her while lying in this condition, and I shall never forget the remark which he made when he was going away. However I did not consider this death-like interval to be entirely attributable to the loss of blood ; hysteria often assumes this still form of existence, and here was a subject under circumstances calculated to excite the most formidable aspect of this disease : the result proved I was right in my exertions. The force of vomiting was at last so strong as to assure me the medicine was producing its effect ; the cloths of the bandage became bloody, which with the sponge were removed ; the latter was saturated with blood.

Life now every hour became more and more established, and in about three months this lady could be removed to the sea side, where she gradually recovered both health and strength. She still continues to be afflicted with the excessive loss of blood occasionally, for which she regularly takes some grains of hippos, but not an ametic dose; and is never disappointed in its effect of arresting the hæmorrhage. The last time I heard of her, she could walk six or eight miles in the day, and that perhaps three times a week.

The four following cases are instances in which the hæmorrhage took place from the alimentary canal.

In May, 1838, a young lady was seized with faintness, while standing beside her brother who was fishing not many yards from the house; soon after getting home she became sick and vomited much blood. The intervals between each attack of vomiting were short and variable, sometimes an hour, and again may be two or three hours. Thirty-two hours from the time of attack the vomiting had not ceased, the patient was lying in the dress in which she had taken ill, loosened as much as possible, as every movement created sickness which would not subside until vomiting came on. Her mouth and tongue were as white as her lips and cheeks from the great loss of blood, the pulse was scarcely perceptible, the extremities cold; had taken nothing but cold drinks in mouthfuls, and her medicine, since the attack came on. In this state the ipecacuan was proposed to be given in grain doses every half hour, combined with same quantity of calomel; after taking the third dose, nausea became distressing, accompanied by faintness; this soon subsided, but after the fourth grain the effort to vomit was complete, and a perfectly yellow bile was vomited without any admixture of blood; the medicine was now laid aside. This lady recovered rapidly and perfectly, under the judicious management of my friend Dr. Cuming, and is now in excellent health.

The second case of this kind occurred in a lady who was in her 85th year; while at dinner I was called to see her,

she was considered dying; about an hour after her dinner she suddenly became weak and faintish, she was cold, pallid, and pulseless, with every appearance of sudden death at hand; soon after swallowing some warm water and brandy, vomiting commenced, and the quantity of blood was such as impressed all with the idea of immediate death. As soon as was possible, I gave her four grains of ipecacuan in a little warm water and brandy, and every half hour one grain in the same fluid; no vomiting was excited in this case, but much nausea, the bleeding was checked, and the old lady, after some considerable time, was again able to resume her accustomed walk in the garden, with the usual assistance of her cane. In three months from this time, a second attack came on, which was arrested by a similar mode of treatment; at present this lady enjoys as good health as generally belongs to the 86th year.

The third case is that of a young gentleman who was attended by the late Mr. Richardson, of Sackville-street, in company with others; æt. 30, a tall, delicate looking man, much addicted to the smoking of tobacco, was seized with vomiting of blood, which continued unrestrained, notwithstanding the closest attention to all the means in ordinary practice; blood also flowed from the bowels. Life was soon terminated, and the evidence of the post mortem inspection was such as to induce me to think, had the ipecacuan been used the hæmorrhage might have been arrested; all the viscera presented a blanched and bloodless appearance, the mucous surface of the stomach ecchymosed in patches, also of the bowels, the ilium particularly.

The last case was remarkable on account of the collapsed condition of the patient, as well as on account of the immense quantity of blood lost, the one depending upon the other. A young lady had been sick in stomach, and fainted more than once the day preceding, but was not willing to frighten her parents by saying she was not well; early next morning, having passed a restless night, she felt sick, rose to ring

the bell for assistance ; while on the floor she became sick in her stomach, her head became light, and it was with some difficulty she was able to regain her bed ; she now vomited a little blood, and became more composed. The vomiting quickly returned and the intervals between each throwing up so short, that her life was in great danger, more especially as she was some miles from medical assistance. On my seeing this young lady, her extremities were cold up to the hips and shoulders, tongue cold, pupils dilated, and with the effort of vomiting a tarry fluid trickled down the angles of her mouth, no pulse could be felt, even the action of the heart could scarcely be discerned ; at once I gave her four grains of ipecacuan with a spoonful of brandy, which with some difficulty was swallowed, and an enema of hot water and spirits of turpentine. About an hour after swallowing the hippo nausea came on, with some slight efforts to vomit, the two first of which were blood-coloured, and the last a colourless frothy fluid. Of course this case, as all cases in which there is much blood lost, required constant attention, but as it is now some time since, the hope of perfect recovery is entertained.*

Dr. Graves saw this young lady some days after the hæmorrhage had ceased, and from the great state of exhaustion in which she then was, could form some idea of the severity of the case.

As I mentioned in the introduction to these short reports of cases, that it was not my intention to enter into the various topics which are embraced in this mode of arresting internal hæmorrhage,—because the practice is not altogether novel, and although recommended by men of experience, and erudition in medicine, there is a something which deters physicians from using a remedy so well recommended to them, and holding out so much success in its use. Now and again cases have occurred, in which the emetic dose of ipecacuan has been attended with unpleasant consequences, one of which is related by Dr. Cullen ; also the apprehension of the blood issuing from an open vessel,

* The hope here expressed has been realized, as this young lady is now able to move about, having regained much of her former strength.

as related by Andral, if I recollect correctly. If such was the case, the use of an emetic dose of ipecacuan must be attended with the very worst consequences. The chief value which the experience of the foregoing cases exhibits, appears to be that the remedy does not require to be exhibited in its full emetic dose, this at once disarms the medicine of many of its disagreeable features. Few friends will administer an emetic dose of ipecacuan in hæmatemesis, even though prescribed by the physician, but let the latter administer small doses of the remedy, and their prejudices are at once overcome ; there is a certain feeling too in the breast of the physician, which forbids him to use a remedy so little in accordance with the ideas of the public, though they may be erroneous, as his success in his Profession depends upon the confidence which the community at large place in his judgment and good sense ; and should these be once forfeited, it is no easy matter to regain them. This feeling, I think, is one great cause why this well known remedy has not been more generally prescribed.

To me it appears both a safe and judicious practice, to administer it in small doses at well timed intervals, a practice full of usefulness and relief to the suffering patient, as well as one less likely to disappoint the physician, than any other I have ever seen used. The after treatment of severe forms of hæmorrhage requires the physician to be well versed in common principles of therapeutics, as there is no form of convalescence so tedious or so difficult, from the complexity of anomalous symptoms which now and again occur, creating much distress to the patient, and requiring a timely and effective management on the part of the physician.

Much might be said upon the subject of the remedies usually had recourse to in the treatment of internal hæmorrhage ; such as the various acids, the combinations of opium with superacetate of lead, ice, cold drinks, &c. &c. ; that these do not afford a constant or uniform relief cannot be denied, yet they are useful in abating the violence of the hæmorrhage. The mu-

riate of soda is also a very popular remedy, and frequently, from its being so ready at hand, one of the first substances used ; now to be useful it must be given in a certain form, that of solution in mawkishly warm water ; it thus excites nausea, and perhaps vomiting : but it is not unfrequently administered in such away as interferes with the saving process of nature. Salt always renders the coagulum of blood fluid, and prevents coagulum from forming, which, with the faintishness and accompanying nausea seems to me to be the natural effort of the economy exerted for its own safety. Used in this manner with tepid water, I have oftentimes found the muriate of soda very beneficial.

ART. XX. — *Observations on Diffuse Inflammation*. By
HENRY KENNEDY, M.B.

[Read before the Medico-Chirurgical Society, November 25th, 1839.]

DIFFUSE inflammation is a disease which has been treated of by many authors. Many points connected with it would, however, appear open for farther investigation : independent of these it is often accompanied by the most exquisite suffering ; it is very rapid in its progress ; it frequently comes on in the course of other diseases both medical and surgical, when every thing else is going on favourably ; and lastly, it is a disease of the most fearful fatality. In this latter respect it may be placed next to hydrophobia itself. For these reasons I have thought it worthy of notice ; and in order to render the following paper as complete as possible, have consulted the writings of numerous authors on the subject.* My own experience too

* The papers written by Duncan, Dupuytren, Lee, Colles, Davis, M'Dowell, Benson, Lendrick, Carmichael, Graves, Arnott, Travers, Rose, Gulliver, Quesnay, Dance, Cruvelhier, and John Hunter, have all come under my notice. Two communications were also made to the Surgical Society last winter, one by Mr. Smyly, and the other by Dr. Beatty. The latter has been published in the last Number of the Dublin Journal.

has been considerable, having seen a large number of cases of the disease within the last five years.

The causes assigned for this disease are numerous: it has followed the most trivial as well as the most severe operations in surgery, the wound of a vein as well as the removal of a limb: owing to it lithotomy is often fatal. A thorn running into the finger, a bruise, a sprain, a burn, a dissecting wound, have all caused it, as also fractures; it has been met with complicated with glanders, scarlatina, and measles; exposure to cold, and bodily fatigue, more particularly when accompanied with depression of mind, appear to be very strong predisposing causes; shocks communicated to the nervous system, as in concussion of the brain, often produce it. In this last case it has been observed for a long period, that when fatal, abscesses are frequently found in the internal organs, particularly the liver. I recollect well the case of a boy who fell from one of the cliffs of Lambay; several of his bones were broken; the poor fellow died within a week, and on examination there was not literally a broken bone or a joint of his body, into or around which pus had not been poured out. The same thing I have seen occur, though not to such an extent, in a boy who fell from the roof of a house. It is quite evident, however, that these causes are insufficient of themselves to produce the disease; they are of every day occurrence, whereas diffuse inflammation is comparatively a rare disease. One cause more particularly has been advanced as producing this disease, viz. venous inflammation; indeed Cruvelhier puts it forward as a *sine qua non*. The following facts, however, induce me to consider the truth of this opinion as more than questionable. In the greater number of cases which I have seen, the veins were healthy, even when they passed through the very centre of the diseased structure; when their coats were thickened, it was evident that this took place secondarily, as their lining membrane was quite healthy. I do not mean to say that true venous inflammation will not be met

with ; I have seen it several times ; but only this, that it occurs in cases which are exceptions to the general rule. Again, five or six instances have come under my notice, where the veins were unquestionably inflamed, and still these cases did not end in diffuse inflammation. Phlegmasia dolens too is allowed by the best pathologists of the day to be caused by venous inflammation, and yet it is a very different disease, both in its progress and termination, from diffuse phlegmon. The limb in phlegmasia dolens presents a very different appearance from anything met with in diffuse inflammation. Take, too, the most aggravated case of the former, and it may be asked, will it cause death ? On the other hand, take a case of the latter engaging the lower extremity, and is there a chance of the patient's recovery ? The fever of phlebitis is allowed by all to be of the typhoid type ; now undoubtedly numerous cases of diffuse inflammation occur, and I have seen such, where the fever was not typhoid, nor anything approaching it ; this will be alluded to further on. For some very valuable remarks on this part of the subject, I beg leave to refer to a lecture of Dr. Graves, published in the London Medical and Surgical Journal for June, 1835. Further, I have seen cases where the disease was confined strictly to the joints, where there was no reason at all to suppose the veins engaged ; and the same remark applies to those cases which occur after fever. Two cases likewise are detailed by Duncan, where venesection caused the disease ; on dissection, however, the veins were found healthy : nothing could be stronger evidence than this in favour of the point at issue. From these facts generally, I think we are entitled to conclude, that diffuse inflammation is not necessarily accompanied or caused by venous inflammation ; and that when this latter does occur, it can be only looked on in the same light as a bruise or a sprain would be, that is, a predisposing cause.

The question of pus circulating in the blood is one of too much importance, and too closely connected with the present subject, to be passed over in silence. The researches of Gulli-

ver on suppuration prove satisfactorily that pus can be detected in certain states of the system in the blood. Indeed Dance long before had made this point all but certain, and his observations are well worthy of attention. The experiments of Cruvelhier too, which prove that when a foreign body, such as mercury, is introduced into any part of the circulating system, even when placed in the reticular tissue of the long bones, that the lungs are among the first organs to suffer, are all important. To my mind the experiments and observations of those three gentlemen are among the most valuable researches of modern times: to what a field for investigation will they not give rise? Gulliver has stated that they will probably account for the different forms of fever, typhoid, hectic, and suppurative, according as more or less pus is circulating through the system, and that hence new views of treatment will be opened up. But will not the experiments of Cruvelhier account for why the lungs are so apt to suffer in typhus, particularly where one of them becomes rapidly solid? will they not throw light on phthisis, showing why the lungs should particularly suffer in that morbid state of the system which produces tubercles, and possibly suggest some remedy for that hitherto intractable disease? I think too, they strongly bear out the mode of treatment which appears to me best adapted to the disease more immediately under consideration, and which will be spoken of farther on.

It has been stated before that the constitution must be out of order, otherwise the disease would not be produced by the various causes spoken of, just as in the gouty diathesis a person may go on a considerable time free from complaint, but should he happen to sprain a joint, that moment the gout declares itself. Of all the ways in which this morbid state of the system shows itself, deranged bowels appears to me to be the most constant: two years since I had the honour of reading a paper before the Medico-Chirurgical Society, on the diagnosis of enteric fever, in which this subject was alluded to; a more extended experience has only confirmed the opinion. Out of a large num-

ber of cases, I have not met a single instance where the bowels could be said to be perfectly natural : if they did act once every day, the discharges were sure to be of a very dark green, often a black colour, and very offensive : in by far the greater number, however, diarrhœa had pre-existed ; the system had been much reduced, and then a something, of which we really know nothing, coming into play, caused this extraordinary disease to appear ; again, the bowels had been confined a week or ten days, and then the disease appeared : the disease which follows on constipation is, however, the most favourable form of it I have seen : it is that which has been so ably described by the late Dr. M'Dowell, under the name of Periostitis.* Although intestinal derangement appears to predispose to this disease, more powerfully than any other, still any thing which has a tendency to depress the system will produce it : thus flooding after delivery is a very common cause, particularly if joined with deranged bowels, but above all, with a desponding state of mind. The natural habits of the patient as to eating and drinking, is the last cause which will only be alluded to here, as there will be occasion to speak of it farther on.

The pathology of this disease is very various : I have seen all the larger joints filled with pus, also the smaller ones, and this may be met, with or without corresponding inflammation of the synovial membranes : the pus is usually healthy, although sometimes it is a thin bloody sanies : when ulceration of the cartilages occurs, it is generally of small extent, in patches, as it were, and *without* surrounding inflammation : in some few instances ankylosis has taken place : it is rare to see the joints alone engaged, the disease, in far the greater number, implicating the neighbouring parts :† indeed, the cellular structure is

* I very lately saw three cases of periostitis, all, however, implicating but a single part of the body ; in two of them diarrhœa preceded the attack, in the third there was constipation : they were all boys.

† For some notes of the following case, I have been indebted to Mr. Benjamin Johnstone. A man labouring under very acute pains, and an attack in his chest,

more particularly the seat of the disease : when affected, it becomes rapidly infiltrated with serum, often of a dark colour, and this is as rapidly followed by the effusion of pus and lymph : in some cases I have seen neither serum nor pus was poured out, but the cellular structure was rendered a solid mass, nothing but lymph being effused : sometimes abscesses formed in different parts of the body.* When the disease spread from the part it first attacked, its course was astonishingly rapid : thus it has commenced at the bend of the arm, spread to the side, and reached the scrotum within three days : again, it has attacked the upper part of the thigh, (and this by the way is a very common part to attack,) and spread to the foot with equal rapidity : in some of the more virulent forms of the disease, the parts attacked ran rapidly into a state approaching gangrene, the constitution appearing to be unable to form pus : in such cases, the very muscles themselves become disorganized : they occur more particularly after delivery, though I have seen them likewise in men. The periosteum is often the seat of diffuse phlegmon, and it is then one of the most painful diseases which it has ever been my lot to witness : the periosteum of the pelvis is very liable to suffer from this form of the disease, but I have seen it also affect the scalp and long bones, in such a way, that the soft parts could be separated from the bones with the greatest facility, and sometimes the epiphyses were found separated. The skin itself suffers but little in this disease, at least in the earlier stages : it very often preserves its natural colour : at other times there is a blush of red-

was bled : the cause of the pains soon became evident : diffuse inflammation of one wrist and thigh joint showed itself, and the patient sank. I was present at the post mortem examination. We found that the hip, in addition to a very aggravated form of diffuse inflammation, had previously suffered from morbus coxæ senilis : the acetabulum was much larger than natural, and also shallower ; the round ligament was gone, and the head of the femur completely altered in shape.

* It would be more correct to say, that purulent depots are found in different parts, for often there is not a trace of surrounding inflammation : the pus seems rather to be deposited in the part, than secreted by it.

ness ; but not to that degree which is seen in erysipelas : this is a point to be borne in mind, as regards the diagnosis of the disease : the redness may also be seen in patches as described by Dupuytren : it may also slough, a state which is not often seen, as the patient dies before this occurs. Vesicles also form, such as are seen in erysipelas : they are by no means common, and pustules form still more rarely. I have sometimes seen large spots of purpura. The pathological appearances are not confined to those which have been detailed : the disease almost constantly attack the more vital organs, causing fearful ravages. The brain I have seen but once attacked : it was the case already detailed in Dr. M'Dowell's paper, where the periosteum of both orbits was affected : in this instance, two or three abscesses were found in the brain : considerable effusion of serum under the arachnoid, and into the ventricles, is however often found. The eye has also been attacked. The parotid gland, with the cellular membrane in its neighbourhood, may be engaged, and then œdema of the glottis is very apt to supervene : at other times, the disease spreads so as to make its way down the anterior mediastinum, and so implicate the pericardium, causing pericarditis : it is curious, that when one serous membrane is involved, the others are very likely to be engaged also : I have seen pure pus effused into the pleura, pericardium, and peritoneum, all in the one patient. The more common effects of pleuritis are very constant. The lungs, which it will be recollected, are the most frequently involved of all the internal organs, present different morbid appearances : you may find them more or less solid, or else in a state of purulent infiltration, amounting even to a state of putrilage : these states are most likely to occur, where the external disease has affected the parietes of the chest : they occur with great rapidity : in other instances, numerous small abscesses will be found containing healthy pus, the structure of the lung round being quite healthy : when, however, the disease runs a longer course, the parts round gradually become condensed, and tubercles make their appearance, or rather tubercular infiltration :

in rarer cases the pus is unhealthy, and the abscesses present a sloughy appearance : these abscesses have been found to occupy the more superficial parts of the lung : as far as I have seen they were also towards the base ; in one instance which I met with, pneumo-thorax resulted from the bursting of one of these into the pleura : masses of lymph will be met with scattered through the lungs, and also evidences of Bronchitis.* In the abdomen, the liver, spleen, and kidneys exhibit the lymphic deposits just alluded to, as also the different abscesses, more particularly met with in the liver : in one case I found the kidneys in a state of purulent infiltration, and as soft as pulp : one ureter contained pus. No organs suffer more severely than those of the pelvis : abscesses of every size and description will be found in and about the uterus and its appendages, coupled with great disorganization of the mucous membrane, particularly the black softening, which I believe was first described by Burns : the veins too are often diseased, and the bladder from its contiguity.† In the intestinal canal, the morbid changes are not so great as one

* One unequivocal instance came under my notice, where purulent infiltration of the lung preceded the effusion of pus, into and around the hip joint. Purulent infiltration of the lung also may be met with alone : I saw one case, where the abscesses of these organs so characteristic of diffuse phlegmon, were the only morbid appearances which could be discovered. These facts, though solitary, are important, connecting as they do, typhoid pneumonia in its various forms, with diffuse inflammation. In Dr. Stokes's able work, mention is made of typhoid pneumonia complicated with diffuse inflammation.

† I have not seen any instance of a coagulum of blood in a vein, and pus in its centre : it has, however, been met with. In the last Number of the *Medico-Chirurgical Transactions*, will be found a highly interesting paper, by Mr. Gulliver, on the softening of fibrine, as met with in both arteries and veins, particularly in chronic diseases : he there shows, that appearances usually attributed to phlebitis, are totally independent of it, and have no connexion whatever with inflammation of the lining membrane of the veins. That these appearances have been met with in cases of diffuse inflammation, and set down as causes of that disease, there can be no doubt : the following is copied verbatim from a late publication on the subject :

“ The uterus was to all appearance healthy, but on cutting into it, the veins were found thickened, and at some distance from the organ were filled with dark

might be led to expect from the symptoms during life: the reason of this is, I believe, because the deranged state of the bowels has in many cases subsided before the patients died: I have, however, sometimes met with ulceration in the lower part of the ileum, and very generally intense vascularity, both extensive and in patches, in different parts of the canal. It will be seen, from the preceding sketch, that many diseases, or at least what have been described as such, have been classed together as one: they all appear to me to bear a sufficient similarity, to allow of this: the tendency of all, is to pour out pus and lymph in different parts of the body at the same time, to afford, in fact, examples of what has been called the pyogenic diathesis, and they all but too nearly agree in their fatal termination.

The disease assumes so many phases, that it will I fear be difficult to give any thing like an accurate description of it: it has run its course in seventeen hours and it has lasted five months; some few cases too have recovered; the cases I have seen myself may be divided into two classes, those in which the constitution has been to all appearance healthy when the disease appeared, and secondly where the system has been run down from any cause whatever. To the first of these belongs the disease so well described in Dr. M'Dowell's paper, under the name of Periostitis and Synovitis; it is a disease of young persons, and is characterized by high inflammatory fever with delirium, an intense degree of suffering and constipation; the state of the periosteum in growing persons may account for the greater frequency of this form of the disease in early life: delirium is a more prominent symptom in it than in any other form of the disease which I have seen. Individuals in the higher ranks of society, who are naturally inclined to be corp-

coagula: the hypogastric and iliac veins were also inflamed, thickened, and lined with the same material."

I have Dr. Montgomery's authority, and none need desire better, for stating that this appearance of coagula in the veins, in the neighbourhood of the uterus, is almost constantly met with, in patients who die of any disease, a short time after delivery.

lent, and to be good livers, are subject to an unhealthy sort of inflammation which is nothing more nor less than a modification of the disease under consideration: it usually attacks the neighbourhood of the rectum, and spreads with great rapidity: in one instance which I knew of, it had reached the axilla before the patient died: the same form of inflammation may follow a slight injury to the shin. I have seen three or four cases of draymen who died from a fearful form of the disease; they were all men of powerful frame, and had indulged largely in malt drink: the parotid gland was the first part engaged, it was attacked with swelling, which rapidly increasing became of a livid colour, and ultimately gangrenous: dark vesicles with sloughs also made their appearance on other parts of the body, particularly the dorsum of the foot: cases similar to these have been detailed by Mr. Travers, and for further information on the point I must refer you to his admirable work on Constitutional Irritation. The effects which sometimes follow dissecting wounds must be referred to this class of cases, occurring as they do in persons in comparatively good health: diffuse phlegmon is however not a common effect of dissecting wounds: when these wounds are serious or fatal, it appears to be caused by the direct absorption of the poison, and not by local inflammation: suppuration does indeed take place, but it is in small quantity, and at an advanced period of the disease. Such are the varieties of the disease which may be met with in persons apparently in good health. They are by no means so numerous as the second class of cases, namely, where the system has been much reduced from any cause, more particularly diarrhoea, before the disease appears: to this class belong the cases which occur after parturition; these offer greater varieties than any other form of the disease: the case of seventeen hours' duration, alluded to before, took place after childbirth; in other instances it has lasted several months: it is in these last cases that anchylosis sometimes occurs. The last to which I shall allude is the disease as seen after fever; and as it is more frequently met with than the others, the following de-

scription of diffuse inflammation will be taken more particularly from it.*

The patient is usually convalescent after having passed through a fever during which the bowels had been in an irritable state : all at once a well marked rigor comes on ; in some cases it is extremely violent, and is renewed three or four times in twenty-four hours : a patient has described it as if some great power had seized them by the small of the back, and shaken them with a degree of violence which they were totally unable to resist : in only one instance have I seen the disease come on without rigor. Dupuytren has remarked that these rigors may be mistaken for intermittent fever, and treated with quinine : I have seen such, and what is curious, the rigor was undoubtedly under the influence of the medicine. Sickness of the stomach and vomiting are also present, and as if by magic the countenance puts on a degree of anxiety, quite enough in itself to point out mischief. At an uncertain period, though generally within six hours after the rigor, the patient complains of severe pain in some part of the body ;† as before stated, about the hip joint is a very common place for the pain to be referred to : when you examine the part you find no swelling nor redness, but there is heat and severe pain, much increased by pressure : the pain may, however, be moderate, and it is by no means uncommon to find swellings in different parts of the body which contain pus, and of which the patient was quite ignorant. Should the disease be confined to the cellular substance, the suffering is comparatively much less than when the joints or the periosteum are engaged : at this early stage the disease may take one

* In a very able Report of the Cork-street Fever Hospital, lately published by Dr. George Kennedy, it is stated, that during the last epidemic of fever, cases of diffuse inflammation were by no means unfrequent, and that in the year 1814 this disease actually became epidemic, so much so as to prevent Dr. O'Brien, the physician then in attendance, from having recourse to venesection.

† I have known three days to elapse between the rigor and the first complaint of pain.

of two courses, it may either spread from the part it has first attacked, so as to involve a whole limb, or else it may attack several parts of the body in rapid succession : this distinction is to be kept in mind, for the former will I believe be found a more favourable case than the latter. When about to spread, some degree of swelling will be found at the end of twenty-four hours ; it is often very slight, but it is not the less important on this account : in some cases which I have seen there was nothing but pain to direct attention to a part where the most serious mischief was going on : the swelling, however, sooner or later, becomes perceptible, increasing till probably the fourth day, when it has reached its height ; it is then very considerable : as far as I have seen, any marked redness of the skin was the exception to the general rule ; in the farther progress of the case Dupuytren has stated that the skin becomes discoloured, and ultimately sloughs, owing to its vascular supply being cut off : it has not chanced to myself to see this take place, but I have frequently seen the disease, after the fifth day, appear to decline, at least as far as swelling went. When the pus is much diffused it is very difficult to detect fluctuation : I have, however, succeeded in making it evident by confining the matter between two fixed points : sometimes pressure on the part conveys strongly the feeling of elasticity.

It can be readily imagined that such a formidable affection as this must cause great excitement in the system : this is, however, not so constant as might be expected ; in about a third of the cases which I have seen the fever was comparatively moderate, the pulse did not range above 90, the tongue was but slightly furred, and the thirst and heat of skin but little increased. It is generally thought the patients labour under typhous fever, but certainly many cases will be met with, where the symptoms of fever are such as have been just detailed up to within an hour or two of death. When the venous system is engaged, the typhoid symptoms are I believe the best marked. The cases where the fever was but slight all occurred in men ; in the

majority however it ran very high, the pulse rose to 130 and even 150, the thirst was very urgent, and the heat of skin very great, the tongue was densely coated with whitish mucus, or else it was dry and red : in some few cases it was morbidly clean, at the same time that it was extremely red ; whatever may have been the previous state of the bowels, it almost invariably occurred that diarrhoea made its appearance in the progress of the case, accompanied by tympanitis to a very marked degree : the discharges were sometimes of a bright yellow colour, at other times dark green or even black, and very offensive ; sometimes the bowels appeared to be confined, but when they did yield, they were sure to become too free. Very rarely indeed did a case go through its course without the chest becoming in one way or other engaged, and yet the patients but seldom allowed they felt any thing wrong ; they sometimes complained of flying stitches, but nothing more, at a time when the respiration was evidently very laborious, and the nares dilated to the utmost. Many cases will be met with where the unfortunate patient assures the medical attendant they are doing well, and that they are quite free of pain ; if any cases of diffuse inflammation can be said to be worse than others, they are these. When an examination is made of the chest, there is usually found more or less evidence of bronchitis, or else the lung may be found solid, having become so in a very short space of time ; when the patients hold out, this solidity gives place to softening, the lung then passing into the state of purulent infiltration, with of course the accompanying physical signs ; in several cases, after the most accurate examination, I was unable to detect any thing wrong, and yet the post mortem exhibited numerous abscesses, such as have been before alluded to. Raving to any high degree was rare, the great majority preserving their mind unusually perfect. The face sank rapidly, often became yellow, and when the disease lasted a week or ten days, the skin put on an appearance analogous to what may be seen in phthisis. The prognosis of the disease must be always unfavourable with

very few exceptions, indeed it is uniformly fatal ; to the cases of recovery which I have seen there will be occasion to allude farther on. The rules for prognosis which apply to other diseases are completely at fault here ; if I were to venture to state any points on which a chance of recovery depended, they would be first, the parts of the body attacked being few ; secondly, the bowels keeping healthy, constipation being considered more favourable than diarrhoea ; thirdly, the patient being advanced in life ; and lastly, the disease being treated from its very onset.

The diagnosis of diffuse inflammation is in general easy, the great and sudden alteration which takes place in the state of the patient within two or three hours is too marked to escape even the most superficial observer ; there is a uniformity about the symptoms, too, which, compared with other diseases, is very characteristic. The well marked rigor, with its frequent repetitions, the anxious countenance, the great rise in the pulse, and the sudden accession of the other symptoms of fever, coupled with the general state of health of the patient previously, are almost all universally present. Let it not be supposed from these remarks that the disease cannot be mistaken, many cases have come to my knowledge where it has not only been mistaken, but what is more strange, completely overlooked. Acute rheumatism can be readily taken for diffuse inflammation, and *vice versa*. In Bouillaud's elaborate work on the Diseases of the Heart, two cases are given which were treated as rheumatism by copious bleedings ; on dissection, however, pus was found in several of the joints : when the disease attacks different parts of the body, but not the joints, it can scarcely be mistaken. I have, however, known one case where it was taken for erythema nodosum ; in another case the disease was supposed to be inflammation of the bursa over the patella, and two leeches were ordered ; in a third the pain which the patient complained of was treated purely as a nervous pain. Phlegmasia dolens and acute anasarca are also liable to be taken for this disease, whether the last form of disease is but a slight degree of diffuse inflammation, I

cannot say ; three cases have come under my notice, and in all it was the lower extremity alone which was affected, the fever was of the irritative kind, and they all ultimately recovered ; the fever which has resulted from dissecting wounds has been mistaken at first for simple fever, as also the pain which the patient has referred to the shoulder joint, whether resulting from a dissecting wound or venesection. An aggravated form of paronychia may simulate the disease very strongly ; lymphatic inflammation has also been stated as liable to be confounded with diffuse phlegmon. Before the disease declares itself the state of the patient may prove very puzzling to the medical man ; the following is a good illustration of this ; for some notes of this patient's case I am indebted to Mr. Gordon and also Mr. Moore : a man about 50 years of age, while labouring under great depression of mind was seized with diarrhoea ; in a fortnight after he was admitted into hospital, complaining of pains in different parts of his body ; on examination no swelling could be detected, nor did pressure increase the pain, his suffering was not at all great ; this state continued for several days, during which the only symptom that attracted notice was uncommon rapidity of the pulse, it beat 140 in the minute, and was extremely feeble ; at last both forearms were attacked with swelling, which extended from the wrist to above the elbows, the right knee and left ankle also became swollen ; the patient sank within forty-eight hours. Mr. Gordon examined the body, and found nothing different from what is usually met with in such cases. Did time permit I could dwell much longer on this part of my subject, but I must hasten on, and shall only remark that but few diseases indeed occur, where a correct diagnosis is of more importance than in this ; a mistake committed must inevitably lessen the reputation of the medical man, the disease being so frequently fatal, and it may lead to the most disastrous consequences as regards the life of the patient.

To the treatment of this formidable affection I would draw attention for a few minutes, not that I have any thing of impor-

tance to communicate, but because it must be allowed that hitherto the best directed and most scientific treatment has failed in far the greater number of instances, and consequently it is the more incumbent on us to give the matter the fullest consideration. Treatment may be divided into constitutional and local. The object of all treatment, in a disease like the present one, is if possible to cut it short at once, or as the French would say, to strangle it; for this purpose when leeching, which is usually first employed, is put in force, it is not enough to order twenty or thirty leeches to be applied to the affected part, to be followed by poultices, and so leave the case for twenty-four hours; such treatment will not succeed in stopping the disease, and what is infinitely worse, it fritters away valuable time which can never be recalled; leeches to be effectual should be applied at least three times in twenty-four hours, of course proportioned to the severity of the attack and the constitution of the patient; by this plan a constant drainage of blood is kept up from the part, and there is a probability of arresting the disease; leeching is usually followed by the most marked relief, even a single application, and it may continue for thirty-six or forty-eight hours; even some cases will be met with, where subsequently to leeching the pain becomes so slight as scarcely to be complained of; let no one however be thrown off their guard by this circumstance, the disease is not the less formidable because it has been rendered latent, and the patient's assertion that he feels much better and is free of pain, must be received with extreme caution; from my own experience I would state that the medical man is not to trust to what his patient tells him, if he do, he will have the mortification of seeing swelling gradually developing itself, for which he was not prepared, and the case running the course which has been before detailed.

The leeches should be followed by poultices, or even cold applications, according to the feelings of the patient. Supposing that leeches are employed, I have spoken of the plan of using them which will be found most efficacious, but I confess though

it is a line of practice very generally adopted, it is not one on which I would place reliance, and for the simple reason that it is seldom if ever ultimately successful; the disease has been certainly delayed by it, but the patients have not the less surely sunk; when the veins alone are engaged leeches however are of the most undoubted advantage. At the end then of eight hours, or possibly even sooner, if the case were a severe one, further treatment should be had recourse to; counter-irritation will come in immediately after the leeching with good effect, but to be really useful it must be of the most powerful kind; it does not surprise me that Dupuytren, in his able paper on the subject, should speak of blisters as being sometimes beneficial but often injurious; the object is to cause a powerful revulsive action on the part, and every one knows that common blistering will not effect this; the actual cautery has proved successful in the hands of Morand, and I believe that on it considerable reliance may be placed. Dr. Evory Kennedy has informed me, that he has succeeded in two instances in stopping the disease, by applying the actual cautery in the very first moment of attack, and when it is recollected that the cases which occur after delivery are almost constantly of a very severe kind, even two cases of recovery become most important. In every case where he has tried it, the immediate effect has been the relief of the severe pain, and also that the disease has been delayed and apparently localized. We know too that the application of the actual cautery is not so painful as might be imagined; unfortunately there exists a strong prejudice against it, which may prevent it being generally used, but where it could be done it should certainly be put in force, and freely; supposing that the parts in the neighbourhood of the hip joint were attacked, or the calf of the leg, four, six, or even eight streaks should be made close to each other, of four to six inches long, and the part then covered with lint dipped in cold water; the parts might be blistered also by means of boiling water, although I am not aware that such a plan has been tried; it is however much more painful than the actual cautery itself.

Neither of the plans of treatment which have been alluded to are, however, to be compared, in my mind, with that of making free incisions through the diseased parts. I am aware that respectable authority has written against this plan, but in this instance authority must give way to experience. Let any one read the numerous cases of diffuse inflammation on record, and they will find that any which have recovered have been treated in this way. It is not contended that the case must do well because incisions are made, but merely that the plan has succeeded where every thing else has failed. In my own experience I have seen five recoveries, and they were all treated by incisions; three of them have been already detailed in Doctor M'Dowell's paper; they were all of the periostitic form of the disease. The fourth case was briefly this: a man about 60 years of age had a moxa applied to the inner side of the knee for some rheumatic affection, the cellular membrane of the whole thigh sloughed, and also a small portion of the skin. In this state he was admitted into Sir Patrick Dun's Hospital, under the care of Dr. Graves, who was at the time Clinical Professor; the case being a surgical one, was handed over to Surgeon Houston's care, and most judiciously he treated this patient; free incisions were made; numerous depots of pus formed over the body, which were all opened as soon as detected: the patient had a most narrow escape for his life, but he ultimately went out well, the wonder of all who saw him. The last was a woman named Mulvey: for an opportunity of seeing this case I am indebted to Dr. John Crampton. She was admitted into the Hardwicke Hospital labouring under fever of a mild form, accompanied by diffuse inflammation of the right wrist and neighbouring parts, and also the left elbow; the bowels had been confined for a week. Dr. Russell made a free incision through the diseased parts over the wrist, and a similar one in the neighbourhood of the elbow. This woman recovered after three or four months with ankylosis of the wrist; the elbow, however, preserved its movements. It may be observed, that under even

the most favourable circumstances the patient's return to health is unusually slow. Independent of experience, reflection would, I think, lead any one to the conclusion, that the treatment by incisions holds out far the best prospect of success : it has often surprized me that the plan is not in more general use. Whatever be the nature of diffuse inflammation, we know that its tendency is to pour out pus with great rapidity, and when left to itself to prove fatal. Now what is more likely to give relief to the system in this state than a free incision, or more properly speaking, to form a large suppurating surface? To this last idea I beg leave to draw your attention, because I am not aware that it has ever yet been acted on ; you will thus establish a channel through which the system may get rid of anything morbid ; and you certainly lessen the chance of deposits of pus or lymph taking place in the lungs or elsewhere, a thing that we know is of very frequent occurrence, and which when it has once happened, must render our best directed efforts nearly fruitless. There are other advantages attending incisions which are much more obvious ; exit is given in many cases to quantities of pus and lymph, a practice which is allowed by all to be of great importance in many and somewhat analogous cases, as for instance, phlegmonous erysipelas, extravasation of urine, and abscess in perineo. It is quite possible too that the disease may be cut short at once, the principle being the same as is put in force for the treatment of anthrax. In that form of the disease where the periosteum is chiefly engaged, incisions are of undoubted advantage ; they give immediate relief from a state of the most exquisite suffering, and if employed very early in the disease, would in all probability cut it short. When periostitis attacks the gums, which it often does with great severity, it can be put a stop to at any period of its course by a proper incision.* In still one other point of view the plan by incisions appears to me to

* Sir Philip Crampton has published in the Dublin Hospital Reports, a valuable paper on the Treatment of Periostitis by Incisions.

be important: till it be put in force you cannot in common prudence exhibit that internal treatment on which so much depends in this disease, namely, stimulants. If you do so, (and I fear much it is done,) you undoubtedly hurry a disease which, dear knows, is sufficiently rapid when left to itself: let, however, any serum, pus, or lymph be given exit to, or let free suppuration be established, and then the necessary internal treatment will come in with good effect; lastly, you may save the skin from sloughing, and if it be thought necessary, you can obtain a considerable quantity of blood from the incisions; in this last respect indeed they require watching. It need scarcely be added after what has passed, that the incisions, to be really useful should be both free and deep: punctured wounds are not only useless, but positively injurious; they increase the irritation without giving relief either to the system at large, or to the feelings of the patient.

Where the cellular membrane is the seat of the disease, incisions give but little pain; not so, however, in the periostitic variety. The reasons are obvious: in the first the knife passes through little if any sensible structure but the skin: in the last it cuts through parts all highly inflamed, to say nothing of the periosteum itself, the most sensitive of all. In order to save the feelings of the patient, whenever it is possible the incisions should be enlarged from within outwards. The place for the incision should, of course, be such as to allow the contained fluids to drain away, for it often happens that at first little, if anything comes out; if this be not possible, the patient may be placed so as to attain the object in view. The incisions are to be followed by stuping and poulticing. It is a curious point to observe the apparent dislike which often exists in the system to form a healthy suppuration; I have seen a full week pass before it was established. Good surgery will have a great deal to say to the ultimate recovery of the patient; but time will not allow of my entering on this part of the subject. When the joints alone are attacked, I fear the case must be looked upon as nearly hopeless; the only chance would be, the meeting the

disease in the very onset by the plan of counter-irritation before spoken of.

The effects of dissecting wounds are in some respects peculiar ; they sometimes produce diffuse inflammation, as any wound may do, and then they must be treated as any other case of the disease would be ; at other times a poison seems to be taken into the system, which by the irritative fever it produces causes death. Two points are worthy of notice connected with dissecting wounds ; one is, that if an individual be in perfect health, he seems scarcely susceptible of suffering from any dissecting wound whatever : and the second is, that when any person is infected and recovers, his recovery appears to be owing to a limited suppuration taking place, and the pus getting free exit.

It is now well known, that when this form of the disease is in an early stage, blackening the inside of the arm with lunar caustic is often sufficient to stop it : indeed, even a ligature round the arm has effected the same end : the internal administration of morphia has also proved of the most signal advantage, as is exemplified in an excellent paper, published by Mr. Stafford, in a late number of the *Medico-Chirurgical Transactions*. This leads on to the internal, or more correctly, general treatment of diffuse inflammation. Should the medical man be called while the rigor still holds the patient, he should do his best to put a stop to it, as there can be little doubt, from the observations of Armstrong, in his admirable work on Typhus Fever, that during a rigor internal congestions occur. A full opiate, with a stimulant and warm drinks, should be given, and heat applied to the feet and spine. An anodyne draught should be kept ready to give, in case the rigor should have a tendency to recur, which is very likely to happen. When the disease is fully formed, various remedies have been tried : they all appear, however, to have failed : general blood-letting has got an extensive trial : I have not seen it tried myself, but from an attentive perusal of the cases in which it has been employed, I think any one must

conclude that it has proved injurious: in Duncan's paper, for instance, numerous cases are given where it was employed to a great extent, and yet in no one instance did it seem to produce any permanent benefit, either to the local or the general symptoms: on the contrary, it may be observed, that in some of the instances, the fever, which before its use was of the irritative or even inflammatory type, became after that of the typhoid. In only one case does the practice appear to me to be justifiable: when it was determined on to try and stop the disease by leeching, and where the fever was inflammatory, as in the form of the disease where the periosteum is engaged, twelve ounces of blood taken from the arm would greatly increase the efficacy of leeches applied subsequently: as to general, or indeed local bleeding, once the part has swelled, and pus and lymph been poured out, they must be considered as highly objectionable. Opium is a remedy of great value in many cases of diffuse inflammation, particularly where there is great suffering, and fever of the irritative kind: it does not appear to have any specific effect on the disease; neither does it affect the head, as in many other cases: it should be given in large and repeated doses. From the great power which mercury possesses over the system, it might be supposed that it could cure this disease, and it has been proposed long since, by Mr. Colles, as a remedy worthy of trial, in the bad effects which result from dissecting wounds: experience, however, does not appear to have confirmed its value: I have seen it frequently used, but never with advantage: in the greater number salivation could not be brought on, and when it was, the disease still pursued its course. One very remarkable case came under my observation, where diffuse inflammation appeared at a time when the patient was strongly under the influence of mercury. When the periosteum is engaged, it appears to do some good, but even here suppuration is very seldom prevented. In the more lengthened cases of the disease, it may be given as an alterative with good effect: it must always be combined with opium. The state of the bowels calls for particular attention:

constipation is to be remedied by the very gentlest means, as by injections of tepid water: if purgative medicine be given, it will to a certainty produce diarrhoea, which, as Dupuytren states, is a symptom of most deadly omen: the tendency to purging is very remarkable: it is not uncommon to find the bowels becoming too free, even while the patient is taking full doses of opium, as if the opium itself produced it. There are, however, some reasons for supposing that a soluble state of the bowels is more beneficial than otherwise: Cruvelhier has several times performed the experiment of throwing pus into the veins of dogs: if the quantity thrown in was large, the animal died: if small, it recovered, and its recovery appeared to be owing to a mild diarrhoea setting in, Nature taking this way of throwing off the morbid matter. Tepid sponging should be employed to the surface of the body, in order to keep the skin in as open a state as possible: one case is detailed by Duncan, where after a most profuse and offensive perspiration the patient recovered. In the more rapid cases of the disease, the pulse must be kept up by stimulants, such as porter, wine, and carbonate of ammonia: porter, however, frequently purges, for this reason it is advisable to add a few drops of tincture of opium to it: the ammonia is, as far as I have observed, a very valuable medicine, and worthy of a more extensive trial than it has hitherto got. When the disease runs a more lengthened course, when it becomes as it were chronic, it would always be judicious, if possible, to change the air of the patient: nourishment should be supplied in every possible shape. By putting these various means in force, both local and general, I feel certain that the fatality of this formidable disease will be somewhat diminished: every thing, however, I do believe, depends upon the promptitude and decision of the surgeon, particularly as regards the local treatment, and during the first few hours of the attack. Finally, from the preceding observations, I would draw the following propositions:—

1. That diffuse inflammation will not attack a person in perfect health.

2. That the bad state of health preceding diffuse inflammation, is powerfully caused by anxiety of mind, by great bodily fatigue, by shocks of the nervous system, by improper diet, or by anything which has a tendency to lower the general healthy tone of the system.

3. That this deranged state of the health is shown principally in a vitiated state of the bowels.

4. That when once this unhealthy condition is established, the slightest cause is capable of inducing diffuse inflammation.

5. That venous inflammation does not necessarily cause diffuse inflammation.

6. That venesection may cause diffuse inflammation, the vein, however, remaining healthy.

7. That when venous inflammation does exist, the fever which accompanies it, is more likely to be of the typhoid type, than when diffuse inflammation exists alone.

8. That diffuse inflammation may attack several parts of the body in rapid succession, or it may be confined to one part, as the hip, or one organ, as the lung.

9. That pus may be poured out into the joints, serous cavities, or cellular structure, without any appearance of surrounding inflammation.

10. That at the very onset of the attack, the free application of the actual cautery holds out a fair probability of checking the disease, but when once formed, free and deep incisions are the only treatment on which any reliance can be placed.

BIBLIOGRAPHIC NOTICES.

The Accoucheur. A Treatise on protracted natural Labours, suspended Animation in New-born Infants, and uterine Hæmorrhage after Birth of the Child, with illustrative Cases.
By JOHN CRAIG, Paisley. 12mo. pp. 252. Glasgow, 1839.

THE title, the convenient size, and some observations in the preface of the above work, would lead us to look upon its appearance as one of the many manuals and safe-guides to midwifery which have issued from time to time from the medical press of these countries. A careful perusal will soon correct these errors, and in place of a succinct account of the acknowledged precepts of midwifery, will be found an assertion of principles, and the details of a practice, as bold as it is opposed to all professional experience; how far this new treatise is a safe-guide to midwifery, the reader shall have an opportunity of forming an opinion. We must first be permitted to congratulate the author on the title of his book; "The Accoucheur" conveys in its very sound such complacent confidence in one's merits, such contempt for an aspiring rival, and such an affected concern for the general welfare, but more especially of your patient, that we know of none which could convey more accurately the spirit of the book. Mr. Craig advances new opinions, and founds upon them a practice different from all his predecessors; the author, therefore, in his preface seems to dread the effects which are to follow his discovery. The great Harvey met with ungrateful cotemporaries; "and what must have been his disappointment when he found, after the publication of his tract, that the little practice he had as a physician by degrees fell off." Jenner while prosecuting his inquiries "was treated with disparagement, and robbed of his merit afterwards." More recently, "Dr. Hamilton has been attacked by Dr. Churchill," (a fact, by the way, new to us); and Mr. Craig seems already to anticipate being added to the martyrs of our Profession. Whatever may result to the author from the appearance of his work, if, like Harvey's tract, it is to raise a host of enemies, and "the little practice he has as a physician by degrees falls off," Mr. Craig

seems determined not to sink unrevenged ; he assumes their hostility, and acting upon the maxim, "the first blow is half the battle," aims right and left, so that scarcely one of his professional brethren has escaped him that come within his reach. Merriman, Ramsbotham, Collins, Breen, Robertson, Maunsell, Churchill, have all received a chastisement according to their merits. Burns hardly escapes, and even Dr. Hamilton, though a brother innovator, and somewhat in the position of a second, occasionally receives a back-stroke. It is, therefore, hardly to be wondered at, that Mr. Craig should have "some misgivings regarding our professional brethren," as they do not generally submit to be smitten on both cheeks. We shall endeavour not to rob our author of his merits, but regret he has given us such little opportunity for commendation.

We cannot approve of the manner in which Mr. Craig treats the experience of men, whose professional eminence requires a little less assertion and more reasoning in controverting their precepts ; neither can we agree in requesting "*the parturient woman*, and those who have a fellow-feeling with her, to peruse these observations with great care," as "the self-evident facts there related will at once satisfy them regarding their truth and usefulness ;" the parturient woman not being exactly in a condition for such studies, and "those who have a fellow-feeling for her," and who are not professional, can know nothing of the truth and usefulness of such facts. Besides, all appeals made to the easily excited fears of ignorant persons, cannot be too strongly deprecated. It looks too a little like quackery to submit a new and bold practice, boasting unparalleled success, and quite in opposition to that generally adopted, to the judgment of those least able to form an opinion upon it.

Mr. Craig's first treatise is upon protracted labour, and without supposing the effects produced by it upon the parturient woman or her friend, we can imagine, what is much more probable, an ignorant midwife, allured by the seductive title of Mr. Craig's work, "perusing these observations with great care," delighted to find in print an opinion which she had long secretly held, as to the folly of sitting out a tedious labour, and rising from its perusal with the fixed determination never in future to attend a case of labour without having a bottle of laudanum in one pocket, and a bottle of "strong waters" in the other. The first case she meets with has kept her longer from her family than she expected, the labour is verging upon the tenth hour without being completed. She at once recollects, with much satisfaction, Mr. Craig's advice :—

"In the treatment of protracted natural labours we are the uncompromising advocates of shortening their duration," [so is she,] "but gene-

rally we are opposed to meddling with any labour *for the first three or four hours* after its commencement. It may happen that on our first arrival, some violent symptom may present itself, which calls for immediate interference, and it should be attended to. But in almost every case, a considerable time should be allowed, to enable the accoucheur to make himself acquainted with the state of his patient, and the true nature of her labour. It will seldom, however, be necessary or proper to defer the means of relief longer *than the period specified*" [three or four hours,] "unless about this time the labour has assumed a character which promises a speedy delivery. . . . At the expiry then of the usual period, if the case has assumed a form which characterizes protracted labour,* the first means of relief we use, is sixty drops of laudanum mixed in a little sugar and warm water."—(p. 39.)

The supposed case has gone nearly to ten hours, and the midwife calling on all the saints to witness her stupidity, immediately administers the sixty drops. It happens to be one of "those cases (p. 39) which really require the employment of laudanum," and consequently,

"Are too obstinate to be effectually relieved by any single means, and we [Mr. C.] find that after the first dose of the opiate has been administered, the pains continue unchanged in regard to their efficiency, though in severity they are often increased."

Such is precisely the effect of the opiate here, the uterine action not being suspended is deranged, and the pains consequently more distressing. To proceed:

"Under these circumstances, about an hour after the first dose of laudanum was administered, a second and similar dose is given to the patient: and should this second quantity fail, in about half an hour after it has been swallowed, to produce a sensible improvement on the labour," [which is the fact,] "we may rest assured that the inflammatory and other symptoms will require blood-letting, in order to remove their obstructing influence over the expulsive efforts of the mother, and bring the labour to a speedy termination."—(pp. 39-40.)

Here the midwife is nonplussed, she cannot bleed, and some one must be sent for who can: wishing in her heart that Mr. C. had the ubiquity of his book, and shunning all accoucheurs of the old school, who might possibly object to the administration of 120 drops of laudanum to a patient, because the labour was lasting something longer than four hours, she sends to the next

* "When the pains in labours of this description continue ineffectual for a few hours, say three or four, they indicate them to be of that kind, which at once comes under the designation of protracted natural labour."—(p. 1.)

apothecary's apprentice, with a request "that he will bring his lancet." The midwife, while waiting the return of the messenger, gets a convenient opportunity of glancing at the work; she finds that,

"Those who may feel inclined to put to the test this mode of treatment must, in order to succeed, be *very accurate* in every step of the proceeding, and *in none more so than in the bleeding and clyster.*"—(p. 42.)

That,

"When the woman is robust," [which she happens to be,] "and the symptoms violent, we should allow the blood to flow, until the face begins to pale, or the patient feels really weaker; in general, before this takes place, from twenty to thirty ounces of blood will be lost." (p. 40.)

Charged with this important precept, the apprentice is desired to bleed until the "face begins to pale."

"About twenty minutes after the patient has been bled, and *without waiting to witness the effects*, a purgative clyster should be administered."—(p. 40.)

This is accordingly done; but as has occurred

"In several of Mr. C.'s 423 cases, instead of the patient being relieved, and the labour expedited, after all the means above-mentioned had been carefully employed, every symptom seemed [seems] to be aggravated, and the woman was [is] thrown into a state bordering on despair."—[p. 42.] The woman now becomes restless, and refuses to be restrained, tossing in bed during every pain."—(p. 43.)

In this unexpected reverse, the midwife, like her patient, "is thrown into a state bordering on despair," and again flies for consolation to the "Accoucheur;" she turns to page 43:

"This is a state of extreme suffering to the parturient woman, and to the accoucheur of correct mind it must be a moment of great anxiety." [What must it be to an upright midwife!]. "If such a state of things be allowed to go on long, all the fearful consequences, so often recorded in similar cases, must inevitably follow. In such circumstances no man, who does not consider himself perfectly acquainted with the nature of a state so distressing, and who is not convinced that the means he is employing are adequate to overcome any present difficulty, could for a moment think himself justified in continuing the same course of treatment, which has effected nothing but an apparent increase of distress. Convinced, however, that he is correct in the judgment he has formed of the nature of his case, he coolly and cautiously determines to subdue the causes of protraction, by increasing

the power necessary for this purpose. In this deliberative mode of proceeding, then, about two hours after the second dose of laudanum had been given, a third dose of sixty drops is again administered, and we need hardly say, that in such a state of suffering, it is not difficult to persuade the patient to take medicine."—(pp. 43-44.)

With this comforting advice, the third dose is given, and we must say, she would be a fortunate patient of whom we could add, "that every symptom becomes changed," and that "the child is born in about an hour and a half or two hours, after the woman had taken the third dose of laudanum." Such happy results are only to be found in Mr. C.'s cases, and we fear that the "sage-femme" who would consider a labour of more than four hours' duration, as demanding interference from its protraction; who would assume that the delay must be "from an unnatural and diseased condition of the parts;" who would give her patient, in consequence, 180 drops of Tinct. Opii, bleed her ad deliquium, purge her in twenty minutes after, because she was bled, and without waiting to witness the effects of the bleeding, we fear she would find reason to regret having "felt inclined to put to the test this mode of treatment." Such, nevertheless, is the substance of our author's practice, in what he calls protracted labour.

Lest we might be thought to exaggerate, we have adopted, as far as possible, our author's words, and we shall now endeavour to apprise the reader of the theory on which the treatment is founded.

Mr. C. divides labours into those in which the uterus is in a "natural or healthy condition," and those where it is "unnatural or morbid."

"Every accoucheur," (says Mr. C.) "even in this country, must have sufficient evidence to convince him of the truth we have now stated, that women occasionally have *very expeditious labours*, and the pains accompanying them are frequently so mild, as to deceive the parturient woman herself, that her child was about to be born, and if we can believe the testimony of travellers and historians, child-bearing in many countries is robbed of all its severities, the children being generally born without difficulty or delay. We are informed too, that among savage tribes, the parturient woman seeks out a solitary place in the woods, to which she retires alone, bears the child, and returns home with it in her arms. *All such cases are so many instances of the natural state of the expulsive apparatus* in the child-bearing woman, and completely nullify the erroneous notions, that violent continued suffering is natural or intended."—(p. 16.)

This Mr. C. calls natural labours, (perhaps we should rather say, labour "in puris naturalibus"), and all which are not expe-

ditions are unnatural, and caused by different morbid conditions of the uterus.

“ The organs concerned in parturition are as subject to diseases in the pregnant state, as other organs or tissues of the body, and just in proportion *to the extent of disease* in the parts affected, will be the delay, the difficulty, and danger attending the labour. Thus it will be evident to every one, that however natural a labour may be in every other respect, it will be rendered protracted by a diseased state of the expelling powers.”

Such causes are summed up under four heads ; 1st, Inflammation ; 2nd, Congestion ; 3rd, excess of sensibility ; 4th, Spasm.

“ These four obstructing causes to delivery, associated with a deranged state of the bowels, are the only distinctions requiring particular notice in the management of these labours.”—(p. 20.)

Mr. C. discards the antiquated causes of delay given by writers, and in fact comes to the conclusion,

“ That the necessity for the use of instruments in so many deliveries of so fatal a description, is occasioned by other causes than the narrowing of the pelvis, or unusual size of the child ; and that these causes, we hesitate not to affirm, are almost always confined to the expulsive apparatus themselves, and are remediable.”—(p. 9.)

On the authority of the Reports of La Maternité, and of Dewees' practice, our author considers that they show,

“ In a very unequivocal manner, that the pelvis is very seldom so narrowed, that it will not allow the delivery of a living child.”—(p. 6.)

The author has shewn in this, as in other instances to which we shall presently allude, “ in a very unequivocal manner,” that he has not read the authority he quotes, in the same careful manner that he claims for his book ; had he done so, he would have found little support from either of those he has quoted.

“ Under the head of *arrest*, which is only another *stultifying name*, employed to denote those forms of labour, in which delay arises from an unhealthy state of the uterine system, in the observations on such cases, much laboured expression has been used by gentlemen of great learning and experience, to explain something *which does not exist* ; and to draw the attention of the accoucheur to the destruction of the child, in order to save the mother ; when in reality, the plain course to pursue is, to remove the obstructing causes to the actions of the uterus, that the infant may be speedily expelled alive, and the mother's safety more certainly secured.”

These extracts are sufficient to show the reader, the cool and confident manner in which "stultifying" epithets and causes (disproportion, narrowing, &c.) which have no existence but in the brains of learned accoucheurs, are disposed of. The learned writers themselves are set aside in an equally summary way. We shall give a few specimens of Mr. C.'s strictures, taking the authorities as they occur to us.

Dr. Denman remarks:

"No means of improving these pains [dilating pains] can be depended upon." "Nothing can be more erroneous, for in no department of the medical art is there a greater certainty of success than in improving the pains in obstinate labours."—(p. 10.)

"Dr. Burns acutely remarks, that, were there no obstruction to the full operations of the uterus, labour would not be protracted. One would think that a mind so constituted as to announce a fact *so brilliant*, would have intuitively suggested a probable and certain means of removing such obstructions, but from what he states, even in his last edition of his Midwifery, it is clear, that he has not yet made a very near approach to a full knowledge of those remedies which should be employed for their removal."—(p. 11.)

Dr. Burns' want of sagacity is only attributable to his being a believer in "arrest" and disproportions of the pelvis. (p. 11.)

"Dr. Ramsbotham has his doubts whether, except in very rare instances, any attempt should be made to palliate them, viz. the pains. These are extraordinary views (says Mr. C.) to be held and promulgated by an accoucheur, possessed of public confidence, and having so many lives of his fellow-creatures, without controul or responsibility, submitted to his care."—(p. 11.)

"Dr. Breen—does not denominate the labour tedious till thirty hours have elapsed from its commencement. Should this worthy Doctor intend any important alteration, either good or bad, after changing the name of labour to that of tedious, certainly no one can accuse him of alarming his poor suffering patient by hurriedly informing her that the name of the labour is now changed, having continued thirty hours." "Had the Doctor employed his time less in uselessly fixing the periods and names of labours, and applied it more assiduously in ascertaining the means suited to the prevention of the extension of labour to *so frightful* a duration, it would be more consoling to the parturient women."

"Dr. Merriman—in his Report states, 'In 9 cases, or 1 in 328, the perforator was employed seven times on account of distortion of the pelvis; two times in very lingering labours.' To what degree the distortions extend, is not so much as hinted at, but from what we have stated above, and *from our own experience*, we can scarcely

admit that they could be all of the description to prevent a child of the usual size to pass with strong efficient pains." [So much for the perforator.] "The forceps also were required fifteen times, on account of morbid conditions of the expulsive apparatus, or what is termed want of expulsive power of the uterus; but this want of expulsive power *never exists except when disease is present.*"—p. 8.

"Dr. Collins.—Although Dr. Collins has put his mode of practice on record, and although it is favourably noticed by the medical periodicals, yet we trust that neither he, nor any one else, will long continue the same course. In, we trust, that it is satisfactorily shown above, that in protracted labours the cause of delay is but seldom, from a narrowing of the pelvis, or an unusual size of the child."

Mr. Craig measures swords with Dr. Maunsell. Both record 423 cases; but with Doctor Maunsell seventy-seven exceeded twelve hours. The perforator was used twice; four women died; forty-three children still-born: "a very considerable number of these were premature births."

All Mr. Craig's cases terminated within twelve hours, with the exception of two, one of which continued thirteen hours, and the other fourteen hours, after the commencement of labour.

"We had only one still-born child; and had we been as well experienced in our present mode of treatment then as we are now, for it was among the first cases, the child, we think, would certainly have been born alive. None of the mothers died."

There cannot be a more flattering contrast.

John Robertson, (Robertson?) Esq., of Manchester, is called to account for having nine cases of ruptured uterus:

"That nine cases of ruptured uteri should come under the care of any man or woman, from any cause, is quite matchless"
Mr. Craig adds, "The causes of protracted labour which we have already enumerated, *we doubt not were the causes of rupture of the uterus*; and we are confident that the mode of treatment adopted by us in such cases will meet the approbation of every unbiassed inquirer."

And we presume, open the eyes of Mr. Robertson to a sense of his position.

Dr. Hamilton alone has had a glimpse of Mr. Craig's discovery. Our author has met with a review of Dr. Hamilton's work, and—

"Was no less pleased than surprised to learn that the great mind of Dr. Hamilton has been directed for thirty-five years to so laudable an object as that of investigating into the causes of protraction in la-

bour, as well as devising and applying means for the removal of such causes."

This is something like the manner in which Mr. Pickwick was complimented when he applied his great mind to the investigation of his theory of Tittlebats; but lest Dr. Hamilton should be too much exalted by Mr. Craig's flattering approval, he is reminded that he has much yet to accomplish.

"We [Mr. Craig] are quite certain that by the mode of treatment we have advised and practised, more cases will terminate within twelve hours after the commencement of labour, than by his [Dr. Hamilton's] method within twenty-four hours; indeed all labours *under our treatment* will be *terminated* by the time or before the doctor has accomplished the dilatation of the os uteri."

Dr. Hamilton appears, however, to have made the nearest approach to Mr. Craig's views, has consequently received a large measure of Mr. Craig's support, and has found in him a very warm advocate. Mr. Craig repels with much vigour a supposed attack by Dr. Churchill on Dr. Hamilton, and derives some consolation from the fact.

"These quotations [viz. concerning Harvey, and Jenner,] with the more recent attack of Dr. Churchill, on Dr. Hamilton, will, we trust, sufficiently, show that risk is run in openly avowing a practice, at variance with professional prejudice; for if men so magnanimous have been attacked and injured by those *who scarcely know what improvement means*; one in our sphere, who has comparatively so little claim to extended publicity and gratitude, would, unless some precautionary step were taken, be almost annihilated," &c.—(Preface, p. viii.)

Mr. Craig's zeal has, however, blinded his perception, the only quotation in his book conveying an attack, and attributed to Dr. Churchill, is the following:

"Again he [Dr. Churchill] states, 'as for poor Professor Hamilton, he must now be accustomed to seeing facts versus his dogmatic assertions. But he has run a muck at every body so often, that he is looked upon by all as an accoucheur Quixotte.'"

This passage, however, does not belong to Dr. Churchill at all, it being the sole property of the Editor of the Medico-Chir. Review. It is clear Dr. Craig's rebuff belongs to him, and he it is who can scarcely know what improvement means, when he had the daring to attack "a man so magnanimous" as Dr. Hamilton. We would recommend to our author in future to read the original of the works he means to censure, rather than the

reviews; in the present instance it would have saved him from rather an unpardonable blunder.

We hope we have said enough to shew the reader the nature of Mr. Craig's treatise on protracted labour, as well as the vigorous manner in which he pushes his opinion over all opposition. Not venturing to controvert them, we shall proceed to the remaining treatises in the work viz. Suspended Animation in newly born Children, and Uterine Hæmorrhage.

Mr. Craig, in speaking of the only still-born child that occurred to him out of 423 cases, states :

“That had we been as well experienced in our present mode of treatment as we are now, *for it was among the first cases*, the child we think would certainly have been alive.”

The natural inference is,¹ that Mr. Craig has met with very few still-born children, and hence the explanation why he can find only one cause, and describe only one form, under which this mournful termination of labour presents itself.

“We have long been of opinion that the principal cause of children born still-born is compression of the umbilical vessels, by the powerful and *unnatural* contractions of the uterus, during protracted natural labours.”

Hence Mr. Craig infers the necessity of shortening the duration of labour, and relieving the child “from its perilous situation.” He also adds :

“From the *relaxed and shrunk appearance* which many, if *not all*, still-born children exhibit, one is apt to imagine that the child has continued to be emptied of its blood by the umbilical arteries, after it had ceased to receive any by the umbilical vein.”

Only that Mr. Craig has given “illustrative cases,” we would suppose that he never met with a case of congestive asphyxia. The only still-born child he had (Case V.) in the 423 cases, seems however an instance of it. There was slight pulsation in the cord at birth, “inflation of the lungs had not been long continued till the lips and then the face and trunk assumed a florid hue, [from being livid,] and soon afterwards the heart began to pulsate strongly.” The heart continued to pulsate several hours, but the child did not make “the least effort to breathe.” Mr. Craig regrets “that he did not encourage the umbilical arteries to bleed,” which he no doubt would have done, but that he had previously determined that

“The purple colour of the skin was caused by a cessation of the return of arterial blood, by the umbilical vein, to the child, before the arteries cease to carry the venous blood out of the body.”

This theory seems to have deprived him of the satisfaction of recording 423 cases of labour without a single accident. There are some points of his treatment that deserve more attention than they generally receive, and none more than the necessity for patient perseverance when inflation is adopted; we know of no means which requires in its use more caution, none that has been more abused by violent and ill directed efforts, and none that should be less hastily resigned; not hastily tying the cord, even though the pulsation may have apparently ceased; and attending to the detraction of the tongue in attempts at inspiration, are also useful hints.

Had Mr. Craig's judgment been a little less obscured by his favourite doctrine of uterine action, we should be saved many objections; but assuming it to be always essential to relieve the child from its perilous situation in the uterus, he seems not at all aware of the possibility of children being still-born from too expeditious a labour. But from the facts he has stated, his range of observation must be limited.

Our limits will not permit us to examine his treatise on hæmorrhage at length. We shall give briefly his treatment of a case of extreme hæmorrhage.

“The first step in the treatment of such cases, which the accoucheur should take, is to administer with his own hand, so as to be certain, not less than 3 iss. of *laudanum* to the patient. In cases of this description, we never give less than this quantity, and we have repeatedly given 3 ii. as the greatest quantity, and without any disagreeable symptom, farther than occasionally, after the flooding has ceased, the patient has been very sick and vomited; which, however, is a favourable result. As soon as the laudanum has been swallowed, a *wine-glassful of two ounces of undiluted ardent spirits, whiskey, rum, or brandy*, should be given to the patient, and the same quantity should be given in as rapid succession as the woman can swallow it, until four wine-glassfuls are exhibited. In a few cases we have given the fifth wine-glassful of spirits, but we have never exceeded this quantity, nor do we believe it necessary.”

Mr. Craig in addition directs—

“The abdomen” [we presume he means the fundus uteri,] “to be well supported by both hands of the accoucheur or an assistant. Cloths wet with vinegar and water to be applied to the thighs, back, &c. The hands to be plunged in cold water, and the face repeatedly sprinkled with the same. The woman should be denuded of bed-clothes, with the exception of a single sheet; and the doors and windows thrown open for the free admission of air.”

These last directions would be unobjectionable, had the necessity for supporting the temperature of the extremities by external means been noticed. We fear, however, that more than one case has been lost by carrying the practice of refrigeration to too unguarded an extent. As to the internal treatment it is another illustration of the use of the bottle of laudanum. The reader can also imagine the importance which, in this country at least, would be attached in favour of the bottle of "strong waters," by the class of practitioners to whom we have alluded. Whatever may be its errors, giving ʒii. of tinct. opii. and five 2 oz. glasses of undiluted ardent spirits "in as rapid succession as the woman can swallow it," is a mode of treatment which is at least not deficient in "promptitude and energy;" and we also agree with the author that it shows in the patient "a remarkable state of the organs of deglutition, which should never be forgotten by the accoucheur."

We hope that we have not treated Mr. Craig's work too unceremoniously, for a new fellow-labourer in the improvement of midwifery; we doubt not Mr. Craig's zeal, and have confident hope that when a more enlarged acquaintance with the researches and facts of his professional brethren moderates a disposition inclining towards dogmatism, he will remove from his book much that is at present offensive. When he learns that the men of the greatest experience, and highest professional attainments, were those who the soonest acknowledged their want of success, and were least disposed to dogmatize, he will understand why it is that positive opinions, confident assertions, and a boasted unparalleled success, is always considered as a presumptive evidence of limited experience. Books are written generally either for instruction or amusement, sometimes for both. If Mr. Craig has failed in the former, he certainly has succeeded in the latter; and as we hope that he will soon be called upon for a new edition, we anticipate many improvements which will enable us to speak of him more favourably.

The modern Treatment of Syphilitic Diseases, both primary and secondary, &c. By LANGSTON PARKER, ESQ. M. R. C. S. L. &c.

Practical Observations, shewing that Mercury is the sole Cause of what are termed Secondary Symptoms. By P. J. MURPHY, M.D., M. R. C. S. I., &c.

WE have classed these two works together, because relating to the same subject, although to our mind they are of very une-

qual value. In medicine, as in other matters, mankind alternate between extremes, and find truth somewhere between.

The thorough-going mercurialists have been met by their proper antagonists the anti-mercurialists, and the true practice is probably to be found in neither extreme, but in the modified system propounded of late years in these countries and in France, and which it is the object of Mr. Parker's little work to explain. With this view, he has given references and extracts from the works of Wallace, Ricord, Lucas, Desruelles, &c., and as confirmative of the opinions adduced, he has added the results of his own experience. The volume is divided into sixteen sections, treating successively of the treatment of syphilitic diseases; of the particular preparations of mercury; of inoculation applied to the diagnosis and treatment of syphilitic diseases; of the different classes of syphilitic diseases; gonorrhœa, chancre, phagedenic ulcers, &c.; of secondary syphilis; of syphilitic diseases of the skin; of syphilitic ulcers; of syphilitic testicle: periorchitis, &c. &c.

The execution of the work is creditable, and we doubt not that, as a manual, it will be extensively read and found very useful. We are sorry, however, to find Mr. Parker adopting the foolish plan of *anglicizing* the new French words, when we have plenty of English ones, which would answer the purpose equally well.

As our extracts must be limited, we shall confine ourselves to those which exhibit the characteristics of the modern treatment—the mercurial '*juste milieu*' school.

At page 11, Mr. Parker asks:

“Why is mercury to be employed in the treatment of syphilis? When is it to be employed? What are the states of the constitution, and of the sore which are to guide us in pursuing its use, or giving it up? And when is it to be discontinued?”

Some answers to these questions will be found in the following extracts.

“Mercury is employed to facilitate the healing of a venereal sore, and to diminish the chance of secondary symptoms.

“Mercury may be used in the treatment of all primary venereal sores, though it is not necessary to their cure, nor is it used in the Paris venereal hospitals, where the sore heals under the use of topical applications, and an antiphlogistic regimen, unless the healing of the sore leaves behind it induration of the cicatrix.

“Mercury is not to be used during the state of fever or local inflammation, which is present during the first days of venereal ulcers,

nor until our patient is prepared for it by low diet, aperient medicines, or local or general bleeding."—(p. 12.)

"When a certain degree of induration accompanies a chancre, or persists after its apparent cure, Ricord has recourse to mercury, and continues he, 'as the mineral is frequently injurious in some other forms of syphilis, so it is of use here.'"—(p. 13.)

"Mercury is employed either in the form of ointment by friction, or internally in various forms of preparations."—(p. 14.)

"During the employment of mercury, the states of the sore, of the constitution, of the mouth and breath, are to be carefully watched, since each of them may assume certain conditions, which would render the further use of mercury injurious."

"It is not necessary that mercury should produce salivation, in order that its benefits, in curing primary syphilitic ulcers, or diminishing the chance of secondary symptoms, may be realized." "The healing of the sore, without a thickened condition of the cicatrix, is our rule for the discontinuance of mercury."—(p. 17.) "We must not think of secondary symptoms, after a sore has healed under the influence of mercury; nor prolong the treatment with a view of preventing them. Secondary symptoms appear after the fullest courses of mercury, and occasionally do not appear when mercury has been altogether abstained from."—(p. 18.)

We should gladly continue our extracts, if our limits would permit. However, we can only recommend our readers to procure the book themselves; it is not a large one, and a perusal of it will afford much information. We shall conclude, by giving a few of the formulæ for the exhibition of mercury.

The Chloride of Mercury is thus exhibited in the Hospital of Val de Grace :

℞ Hydrarg. Chloridi.
Extracti Conii, āā. gr. i.
Pulv. Glycyrrhizæ, q. s. ut ft. Pil.

One or two of these may be given in the day, and gradually increased.

Ricord's form is :

℞ Hydrarg. Chloridi, ℥i.
Pulv. Conii.
Sapon. Hispan. āā. ℥ii.
M. ft. Pil. xxiv.

The following are the formulæ for the bichloride :

"At Val de Grace.

℞ Hydrarg. Bichlorid. gr. $\frac{1}{4}$,
Pulv. Opii, gr. $\frac{1}{2}$.
Pulv. Glycyrrhizæ, q. s. ut ft. Pil.

One a day for the first ten days, the dose then gradually increased.

“ Dupuytren’s Form.

℞ Hydrarg. Bichlorid. gr. ii.
Pulv. Opii, gr. viii.
Gum. Guaiaci, gr. xxxii.
M. f. Pil. xvi.

One of these is given three times a day.

“ M. Cullerier’s form of the Proto-ioduret of Mercury.

℞ Hydrarg. Proto-iodureti, gr. xii.
Extr. v. Pulv. Opii, gr. vi.
Gum Guaici. ʒ i.
M. ft. Pil. xxiv. Capiat i. nocte maneque.

“ Gargle of the Cyanuret of Mercury.

℞ Hydrarg. Cyanuret, gr. x.
Infus. Lini Co. lb. i. M.

“ Pills of the Cyanuret of Mercury.

℞ Hydrarg. Cyanuret. gr. viii.
Pulv. Opii, gr. xvi.
Ext. Guaiaci, ʒ ii.
M. f. Pil. lxiv. cap. i. ter die.

“ Ointment of the Cyanuret of Mercury.

℞ Hydrarg. Cyanuret. gr. xii.
Adipis, ʒ i.
M. f. Unguent.

“ Solution of the Cyanuret of Mercury.

℞ Hydrarg. Cyanuret. gr. vi. ad gr. x.
Aquæ, lb. i. M.”

As Mr. Murphy is an Irishman, we shall make no apology for committing an *Irishism*, by commencing our notice of his work with its conclusions, as the best means of laying before our readers the doctrine he advocates.

“ 1. That the secretion which produces gonorrhœa is identical with that of chancre.

“ 2. That consequently as early an origin must be assigned to chancre and bubo, as to gonorrhœa.

“ 3. That bubo arises from inflammation of a lymphatic vessel continued to its gland, and not from the absorption of a peculiar virus.

“ 4. That ulceration of the throat, cutaneous eruptions, iritis,

nodes, venereal swelled testis, &c., and termed secondary symptoms, are produced solely by mercury.

“ 5. That chancres and bubo are simple local affections, and consequently not followed by any constitutional symptoms.

“ 6. That the peculiar train of symptoms termed syphilis in infants, have [has?] two sources—a mercurialized constitution transmitted by a parent—and sibbens.

“ 7. That sibbens not having been properly distinguished from chancre and mercurial ulcers, is one great cause of the obscurity which surrounds the venereal disease.”—(p. 103.)

Connected with these conclusions is another series enumerating the ill effects which Mr. Murphy says he has traced to the use of mercury.

“ 1. It originates scrofula, transmissible to the offspring.

“ 2. It excites tubercles into action.

“ 3. It leaves the body susceptible of nodes, rheumatism and other diseases of the fibrous system.

“ 4. It produces a tendency to ulceration of the surface and throat.

“ 5. Dysentery and ulceration of the intestines.

“ 6. A predisposition to aneurism.

“ 7. Anterior capsular cataract as a consequence of iritis.

“ 8. Caries of the teeth with severe pain, mistaken for tic-douloureux.

“ 9. A peculiar species of apoplexy.”

Although we cannot agree with Mr. Murphy in several of his conclusions, yet it is but just to say that his reasoning is vigorous and plausible, and his style clear. We should have been better pleased had he written with less confidence on so difficult a subject. Doubtfulness is not necessarily a proof of limited knowledge.

As we firmly believe that truth is a reality, and that it is discoverable, so do we rejoice in the appearance of such works as this, inasmuch as by investigating the subject thoroughly, testing every fact, examining every opinion, and throwing overboard the influence of great names, we get rid of much that has caused obscurity, and shall ultimately arrive at a correct judgment.

Before we conclude we must congratulate Mr. Murphy on his having preserved certain national characteristics, notwithstanding his transplantation to an English soil. We could point out several transpositions of “shall” and “will,” “would” and “should,” and the following is certainly “very like a bull:”—

“Of the treatment of gonorrhœa *it is not my intention to speak, but avail myself of this opportunity to say, that in the inflammatory stage, leeches to the urethra are highly beneficial,*” &c. &c.

SCIENTIFIC INTELLIGENCE.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.—(CONTINUED.)

November 7th.

Dr. BREEN, V. P., in the Chair.

The first meeting of the Society for the present session was held on the 7th November, when the following opening Address was delivered by Dr. Breen, one of the Vice-Presidents :

GENTLEMEN—When my respected friend Dr. Doherty, Secretary to the Dublin Obstetrical Society, applied to me, as a veteran obstetrician, and I believe the senior practitioner among the Vice-Presidents of the Society, to take the chair on this evening, I willingly consented, in order to mark strongly my opinion of the benefits conferred on the Profession by such an Institution ; and, at the same time, expressed my willingness to say a few words on the utility of the Society, as well to the senior as to the junior obstetricians, who had the opportunity of attending, and availed themselves of it by being present at our meetings.

From the period that men of education began to devote their time and talents to the practice of midwifery, Dublin has constantly possessed its full share of eminent individuals in this branch of medicine. The early formation of one of the largest Lying-in Hospitals in the world, in this city, that now existing in Great Britain-street, almost within whose walls we are assembled, naturally gave a stimulus to the cultivation of obstetric medicine. The Mastership of that Institution, always a situation of emolument, and a just object of ambition to those who, under the charter, were qualified to obtain that place, excited the candidates earnestly to cultivate this branch, both to obtain the support of the governors, who had the power of appointing, and to secure the patronage of the public, without which eminence in practice could not be obtained. The necessary consequence of this state of things, then as now, was, that gentlemen unconnected with the Dublin Lying-in Hospital devoted themselves to acquire a knowledge of midwifery, rose to eminence in obstetric practice, and enjoyed a large share of public favour. In more distant times Sir Fielding Ould, Master of the Lying-in Hospital, and Doctor Foster,

an assistant to that establishment, published treatises on midwifery. In Sir Fielding's work a more correct view is given of the mechanism of the passage of the head through the pelvis, than had before been given. Indeed from the time Mauriceau wrote, who overturned the erroneous opinion that the ligaments of the pelvis became softened, and yielded in difficult labour, no improvement was made in the mechanism of parturition, from that period to that of Ould. The notion of the yielding of the ligaments led Chamberlayne, the inventor of the midwifery forceps and vectis, to imagine that he could deliver with the former instrument in every difficult instance of head presentation, and led him to try the forceps in a case of very deformed pelvis at Paris, where he had gone to sell his secret of the forceps. The failure of the experiment, which caused the death of both mother and child, proved his ignorance of the mechanism of parturition, and blasted his hopes of reward from the French government. The incidental mention of the mechanism of parturition leads me to suggest, that this subject is yet open to the zealous investigator, much yet remains to be done ; and though Douglas of Dublin, and Naegelé of Heidelberg, have thrown light on this particular branch, the field of inquiry is still very open. Doctor Foster's treatise on midwifery appears to me chiefly remarkable for, as far as I know, being the first treatise which recommended small doses of tartarized antimony, frequently repeated, in combating puerperal fever. This practice for many years was a good deal followed in this city, and has never, under certain circumstances, been given up, in the treatment of this desperate malady. Of distinguished men, not connected with the Dublin Lying-in Hospital, and who practised midwifery, I may mention the names of Dr. Mac Bride and Mr. William Dease. The first, though he wrote little on this branch, in proportion to what he wrote on general medicine, attained great eminence as a practitioner of midwifery in this city ; he had the high distinction of attending the Countess of Mornington, at the birth of the hero of Waterloo. At a contested election, when the then Arthur Wellesley was elected member for the borough of Trim, a petition was presented against the return, on the ground that the returned member was not one and twenty ; some difficulty at the moment occurred in proving his age, and the late Dr. Evory, who possessed Dr. Mac Bride's fee books, attended as a witness before a Committee of the Irish House of Commons, with Dr. Mac Bride's entry of the birth of this distinguished individual, and, in addition to other evidence, the age was admitted legally proved by the Committee.

Mr. William Dease, commonly distinguished as Mr. Dease, primus, in the latter years of his life known in Dublin as a very celebrated surgeon, and one of the first Professors of Surgery to the Irish College of Surgeons, in early life practised midwifery, and wrote a treatise on the subject, the chief distinction of which appears to me that he was the first who suggested the use of the midwifery vectis, as an extractor, making the fulcrum one hand of the operator, and not allowing any part of the pelvis of the mother to be used for this pur-

pose. The distinguished obstetricians who have followed these four eminent men, have I believe been brought under the notice of this society by the talented individual who took the chair at the opening of the last session. The older practitioners then noticed were my cotemporaries in early life, and the younger ones still continue, and I hope will long continue, to cultivate and improve obstetric medicine; therefore, I am not called on to mention their names or claims. Before passing from this subject, I may glance at an objection that I have heard mooted against this society. It has been hinted to me that some eminent obstetricians in Dublin are not quite free from an opinion that it has been got up to serve a particular school. Were that the case I would not be in the chair this evening, nor would my learned friend, Doctor Churchill, be prepared with a paper for this meeting. For myself, I can say that my object in attending is to contribute my mite to improve our art.* In alluding to what was done at our meetings last session, I regret to say that my occasional, and sometimes necessary absence, through professional engagements, will probably be the cause of my omitting to notice many important discussions; as, though a sketch is given of the proceedings at the different meetings in the Dublin Journal of Medical Science, it necessarily falls very short of the impressions conveyed at the discussion to one who is present. I must therefore apologize to those gentlemen whom I had not the good fortune to hear, on account of unavoidable absence from some of the meetings of last session. Before alluding further to the practical value of the information derivable from the society's past labours, I will beg leave to point out a peculiarity in this institution, with regard to the benefit to be derived from it by the younger members. If I can take the result of inquiries extensively made among a great number of practitioners of midwifery, with whom I have been on terms of unreserved communication, I have never met with one who would not have preferred eminence in any other branch of medicine to eminence in midwifery, were it equally attainable, or within probable reach; in a word, who would not have selected Doctor Purcell's, Doctor Percival's, or Mr. Richard's station, in preference to that of their cotemporaries, Doctor Clarke or Doctor Evory. This feeling in my mind leads the young obstetrician not to cultivate midwifery with as much ardour as he does medicine and surgery. I therefore hold that at meetings, such as we now witness, where the ingenuity of the young, the practical steadiness of the more advanced investigator, and the matured experience of the old practitioner, are willingly unfolded, the junior obstetrician must derive with little loss of time much useful knowledge; and when I slightly glance at the investigations of last session, it will be seen that the old may be much improved and instructed by the informa-

* Could a notion prevail that our Society was in any way exclusive as to school or nation, some letters placed in my hands from distinguished individuals, far removed from our local feeling, will set the subject at rest. Read Sir J. Clarke's letter and others, numbered 1, 2, 3, 4.

tion unfolded in this room. I heard exceedingly interesting discussions on the protective power of vaccination—on the occurrence of small-pox after vaccination—on uterine hemorrhage—on Mr. Hugh Carmichael's ingenious opinion on the more usual place of attachment of the placenta to the uterus—on the use of the ergot of rye, and on what I mean to dwell longer on, the separation of the cervix uteri, or a part of it, in labour. It is curious that though this occurrence cannot be so rare as the few recorded cases would lead us to believe, that the first English case, that of Mr. Page Nichol Scott, published in the year 1821, in the *Medico-Chirurgical Transactions*, and abridged in the *London Medical Repository* for 1822, the editor of which journal, I think Doctor Copeland, states that one similar case is recorded in some foreign journal, but does not mention the year. These cases almost seemed to have fallen into oblivion, or to have been thought so exceedingly anomalous, as to have been rarely referred to. The occurrence of three cases in this city, within a short time, shows that the fact must have been perhaps frequently overlooked. If any of my audience inquire why I dwell so much on these facts, my reply is that I think they prove that the cervix uteri performs a different function from the fundus; that though continuous, it is independent in structure; and that some practical consequences result from this view. Thus in inversion of the womb, which has never been replaced after a few hours' continuance, a suggestion arises, might it not be advisable to divide the cervix, which seems, by its close contraction on the fundus, to prevent its replacement. Should this operation prove successful, the woman is restored to all her natural functions, and is capable again of propagating her species. While the removal of the fundus, either by excision or ligature, though it relieve her from much suffering, and exhausting hemorrhage, leaves her unfit for important feminine duties. In addition, I rather think the recorded cases took place in young subjects. An ingenious friend of mine, Mr. Power of the Coombe Hospital, has also suggested, that, in the very deplorable case of impending dissolution from retained placenta, we might be warranted in trying the division of the cervix. This elucidation of the independent structure of the cervix seems also to have a connexion with explaining the principles of Doctor Hamilton's practice, of Edinburgh, in dilating this part in labour, threatening to be tedious. It will also, as it appears to me, assist in the due investigation of the accuracy of Mr. Hugh Carmichael's opinions, as to the more usual attachment of the placenta. Human ovology, a department of obstetric medicine now zealously cultivated, as well in this city as at most other places where midwifery is taught, offers a fine field for the junior obstetrician to investigate and consider the wonderful works of the Creator. It seems with the kindred subject of inquiry, physiology and pathology, united with that scrutiny whose paramount importance no man can rationally doubt, a scrutiny into the grounds of our belief in revealed religion, to form the most perfect circle of our knowledge of the secrets of creation.

When the ovologist looks on the foetal rudiment, and reflects that such he once was, when assisted by physiology he considers the functions his frame now performs, when bringing pathology to his aid he traces the germs of decay and dissolution; by the light of revelation he is more thoroughly let into the secrets of the Deity, with regard to himself and his fellow men, than the investigator into any of the other wonderful works of the Great Architect. The astronomer, who reads the heavens with his telescope, and receives the ray of light on his object glass from a fixed star, invisible to the naked eye, may calculate the centuries it took to pass through space, and conjecture that it proceeds from a luminous body, like our own sun, the centre of a system; but his opinions are nothing but conjecture. The geologist may trace the changes of our globe, as to its preexistence before our era; but his investigations are still clouded by doubts. It is probable that the sages of India and China, who in far gone times held a belief in the existence of a future state, derived the opinion from the tradition of the immediate descendants of Noah, and not from their own reasoning powers.

Though the rudest savage, relieved from the mere pressure of the want of his daily subsistence, when in contact with his more civilized fellow-creature, at once admits himself an accountable agent, and by an almost unavoidable consequence, acknowledges his acquiescence in the doctrine of a future state; though the talented metaphysician may have exhausted the utmost ingenuity to prove the separate existence of the soul from the body, and its necessary immortality, as a spirit, the arguments are all inconclusive without the aid of Revelation; the resurrection of Jesus Christ, winding up the promises of the old law, proving both his divine mission and the truths of the doctrine he taught, and demonstrating on irrefragable and irrefutable evidence a future existence. It is thus I assert that the labours of the physiologist, and the investigations of the inquiring Christian, throw light on the subject of our future state. The first suggests such a state as conformable to reason, and to the constant improvement of the state of man. The second confirms the existence of that state with a certainty which no reasonable person, who investigates the subject, can doubt.

In the fulness of time, the number who will partake in the happiness of this state, in conformity with our opinions and experience of the infinite goodness and mercy of our Creator, is recorded in the last of the series of Divine Books, as "a great multitude, which no man could number, of all nations, and kindreds, and people, and tongues."

At the conclusion of this Address, a paper was read by Doctor Churchill on the History and Use of the Lever, which will be found amongst the original communications of the present Number.

Doctor Ireland then rose to make a few remarks upon a point which related to the use of the instrument; one he considered of material importance. He understood Doctor Churchill to recommend

the lever when the head remained at the brim of the pelvis. He begged to ask that gentleman whether he had ever used it under such circumstances, as, in his mind, he thought it not at all applicable before the child had entered the pelvis, or until it came almost to rest on the perineum.

Doctor Churchill had not had experience of its use at the stage alluded to, nor should he then himself employ it; and only gave the statement on the authority of certain authors.

Doctor Ireland merely asked the question to quote Mr. Dease's words, that when gentlemen get a little more experience they will let their instruments rest in their cabinet; and this he did on account of the young men present, lest they should resort to the application, so as to make it a curse to their patients instead of a blessing. He was sure Doctor Churchill's information and experience prevented him falling into error; but he feared some gentleman present, less qualified to form a correct judgment, might follow the suggestion thrown out, and produce irreparable mischief.

Doctor Churchill alluded to the chairman, Doctor Breen, having formerly written upon the subject, and appealed to him and to Doctor Kennedy for the result of their experience upon it.

Doctor Breen remembered one particular case, given in an early paper of Doctor Hamilton's, inserted in Duncan's Commentaries, in which, owing to slight deformity of the pelvis, the head rested at the brim. He used the lever, and delivered a living child. This is the only case Doctor Breen was aware of. For his own part, he should not use it except when the head was on the perineum.

Doctor Kennedy, as he was appealed to, would state that he had tried the lever with the head entering the pelvis, and failed with it, unless when the pelvis was roomy, and the foetal head small; even when the head is engaged lower in the pelvis, in its transverse diameter, it is very difficult to bring it down with the lever; and it appeared to him principally applicable in cases of arrest from inefficient action, with the head approaching the outlet. Even when there exist the ordinary proportions, he thought that delivery will be effected better and more safely by the use of the forceps, particularly if assistance is required at an earlier stage than that described. He could not understand the possibility of delivery by the lever, with the head at the brim of the pelvis. When it is requisite to introduce the lever so far, it becomes buried in the pelvis, and loses its power. The long forceps is the proper instrument to employ there, if interference be justifiable. In the application of the lever, it should be used not as a lever, but as a tractor; the fingers of the left hand should be introduced so as to form as it were the other blade of a forceps, and with the thumb of the same hand the instrument should be grasped at the junction of the blade and handle. Thus the head can be drawn down more efficiently, and with less risk of injury to the mother.

Doctor Ireland frequently used the lever with great benefit; and in very many cases found that the mere stimulus of its introduction was sufficient to rouse the uterus into increased action, and so accomplish the delivery.

Extraordinary Case of a Woman in Dublin pregnant of five Children.—Doctor Evory Kennedy produced five fœtuses, with their involucra, the product of a single abortion, at the meeting of the Dublin Pathological Society, held on the 14th instant. The patient had been attended by his late assistant, Doctor Thwaites, and pupils of the Hospital, and the facts of her case accurately noted, so that deception was impossible. The specimen produced, Doctor Kennedy stated to be the multiparant conception of a female, who aborted when, as she stated, she was three months' gone with child. The case was one in which there appeared to be three distinct ova; two of these were twins, the third was single; so that five fœtuses coexisted in utero. On examining the preparation which was now exhibited to view, Doctor Kennedy remarked, that more closely it would be found that those on each side differed from the centre one. Each of the former possessed a common placenta, and membranes common to both, with an intervening septum; but the central one is distinct and perfect in itself, having its own placenta and membranes. Some persons have been disposed to question the occurrence of these multiparous births; and indeed it must be acknowledged that the popular opinions, and even recorded cases, on the subject are sufficiently extravagant; as for instance, the Countess of Hannenberg's case, in which it was stated that 365 children were produced at a birth. But without taxing our credulity in these cases too far, we have undoubtedly a few well authenticated instances on record, in which women have given birth to five children at a time. One of these, that of Guiseppa Califani, occurred lately at Naples; and we have the details of another, which took place in Franklin County, in America, about twelve years ago, recorded by Doctor Paddock. There is also said to be a similar preparation in the British Museum. It is extremely curious and interesting, as connected with the history of multiparous births, that in this respect Ireland preponderates over all other nations, and that the Irish females are unequalled in the ratio of their fecundity. The proportion of twin cases in Dublin is one in sixty; in America, (where, it is to be recollected, there is a large number of Irish emigrants,) the proportion is one in seventy-five; in London it is one in ninety-one; while in France, "*longo intervallo*," it is one in 140. In proof of the rarity of *quinque geminii*, Doctor Kennedy further remarked, that out of 140,000 cases recorded in the Lying-in Hospital of Dublin, there is no instance of five children at a birth. There is one case of four, but none of five. It is a curious fact, that in the American case the mother was an Irish woman, and had recently arrived in America. It may perhaps be considered equally curious that in the case detailed by Doctor Kennedy the father was a man of small stature, æt. about thirty, without any remarkable personal development, and by trade a *tailor*! The woman, the subject of the present memoir, whose name is Sarah Hickey, is twenty-eight years of age. She was married about two years ago, and within nine months after brought forth her first child.

This conception was uniparient. After the lapse of six months she again conceived of the fœtuses alluded to; and observed that during the pregnancy she increased very rapidly in size, and suffered constantly from bearing down, which rendered walking or standing almost impossible. She had constant sickness of stomach—a symptom generally looked on as an evidence of compound pregnancy. As to the abortion, it would appear to have been produced by inordinate distention of the uterus for its period, which in its turn led to parturient efforts, as the ova presented no morbid appearance. The fœtuses, which are all males, do not appear to exceed the development usually observed about the second month; and as Hickey menstruated on the 24th May, and miscarried on the 26th August, it is more than probable she over calculated the duration of her pregnancy. This preparation is in Doctor Kennedy's museum, in the Dublin Lying-in Hospital.

Case of Uterine Hæmorrhage, in which the Blood escaped through the Fallopian Tubes, by W. F. Barlow, Esq.—A young woman, twenty-two years of age, who had miscarried at the sixth month, with much flooding, had an attack of purpura hæmorrhagica, from which she died five days after abortion had taken place. On dissection, a quantity of blood was found to have been effused into the abdomen and pelvis, some of which was coagulated; and, as clots were found projecting from the fimbriated extremities of the fallopian tubes, from which, as the author considers, it was evident they had been expelled, he infers that uterine hæmorrhage had taken place through these tubes into the abdomen. A preparation of one of the tubes, having a lobulated coagulum projecting from it, was shown to the meeting, together with a drawing from it when recent. The uterus was less than the size it generally assumes a week after delivery, and a coagulum of blood partly occupied the neck of the organ. —*London Medical Gazette*, Dec. 6, 1839.

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